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NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

(ACRS)

REACTOR FUELS SUBCOMMITTEE

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WEDNESDAY

OCTOBER 9, 2002

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ROCKVILLE, MARYLAND

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The Subcommittee met at the Nuclear Regulatory Commission, Two White Flint North, Room T2B3, 11545 Rockville Pike, at 8:30 a.m., Dr. Mario V. Bonaca, Chairman, presiding.

COMMITTEE MEMBERS:

DANA A. POWERS	Chairman
MARIO V. BONACA	Member
F. PETER FORD	Member
GRAHAM M. LEITCH	Member
STEPHEN L. ROSEN	Member

ACRS STAFF PRESENT:

MEDHAT EL-ZEFTAWY

OTHER NRC STAFF PRESENT:

SUDHAMAY BASU

RALPH MEYER

JACK ROSENTHAL

HAROLD SCOTT

UNDINE SHOOP

JARED WERMIEL

EPRI REPRESENTATIVES PRESENT:

ROSA YANG

ROBERT MONTGOMERY

C-O-N-T-E-N-T-S

P-R-O-C-E-E-D-I-N-G-S

(8:32 a.m.)

CHAIRMAN POWERS: Let's come into session here. This is the ACRS Subcommittee on Reactor Fuels. I'm Dana Powers, Chairman of the Subcommittee. The ACRS Members in attendance are Mario Bonaca, Graham Leitch, Jack Seiber, Steve Rosen and Peter Ford.

Before I get into the introduction to the meeting, I do have an announcement of interest perhaps to the Members of the Subcommittee, is that Jessie Delgado is inviting you all to attend the Fourth Annual Hispanic Month Dinner, which is being organized by the Hispanic Employee Program Advisory Committee in celebration of Hispanic Month. It will be held at On The Border Restaurant, 1488 Rockville Pike at 6:30. The cost is $20 which includes meals, dessert, and a non-alcoholic beverage. I understand Chairman Meserve and Commissioner Diaz will be there. If you'd like to attend this dinner, see Jessie before noon so she can get you a menu selection and give you information on how to get to the restaurant. I think all of you will find that an enjoyable experience.

Today's meeting has a lot of stuff that has to go on the record for format sake. First, I'll note that Med El-Zeftawy is our Cognizant ACRS Staff

Engineer. The rules for participation in today's meeting have been announced as part of the notice of the meeting previously published in the Federal Register on September the 23rd, 2002. A transcript of this meeting is being kept, and will be made available, as stated in the Federal Register notice.

It is requested the speakers first identify themselves, and speak with sufficient clarity and volume so they can be readily heard. We've received no written comments or requests for time to make oral statements by members of the public.

What I'd like to do is a little introduction on the strategy that we want to pursue here. We're going to talk today about the Reactor Fuels Program and some of its results, focused primarily on the behavior of high burn-up fuels under design- basis accident conditions. We're not going to discuss reactor fuels pertinent to the advanced reactors, per se.

Consequently, this discussion would not be part of our research report, so we need to discuss whether we want to prepare a letter to the Commission about this particular research program or not, so bear that in mind as we progress through the discussion, especially this afternoon when we hear about the research program per se.

I think the other things that we're not going to discuss are high burn-up fuel in beyond design-basis accidents. That's another aspect of the program that's not being presented here today because that work is in some early stage of development and cooperative research. Be aware that there is - I'm looking at high burn-up fuel that goes well beyond design- basis accident considerations.

WE also need to consider what information needs to be presented to the Full Committee about these programs. High burn- up fuel has an influence in quite a number of issues that come before the Committee, beyond just the fuel research program itself. Certainly, we're going to hear about high burn-up fuel in consideration with transport casks and on-site storage.

We've already had discussions of high burn-up fuel in connection with power uprate program where there's reasonable confusion in my mind on exactly what is being used as the enthalpy limits on the fuel. So as we progress through today's presentations, the Members should think about advising me on what it is that we want to present to the Full Committee so we keep them up to speed on what's going on in the world of high burn-up fuel, because it impacts a lot of things we discussed.

Today's program requires some introduction, if you're not intimately familiar with what all has gone on in connection with high burn-up in the past. I think everybody understand that licensees have a tremendous economic incentive to use fuel to as high level burn-up as safely possible. It's important also to recognize there is a tremendous societal incentive to use fuel at high levels of burn-up. I mean, quite frankly, the less fuel one uses, the less spent fuel there is that one has to store on-site, the less fuel that has to be disposed in some geological repository, if it ever gets constructed. So the question is, how far can we take the fuels that we have safely in the current generation of reactors?

And it probably comes as no surprise to you that the limits to which we've allowed fuel to be burned up have quickly exceeded our empirical database in understanding how fuel behaves under upset conditions. The limitations on that understanding has been brought to our attention abruptly by a series of tests that have been conducted in Japan, in France, and even in Russia on the responses of fuel to reactivity insertion.

As a result of those experimental results, the agency has put a limit on the level of burn-ups that it will allow fuel to go without some further justification, and an agency-wide research program was initiated to confirm that, in fact, this limit still preserve the public health and safety, and that really is the research program that we're looking at.

We're also going to get to hear some discussions of analyses of these reactivity insertion events that -- reactivity insertion tests that have been done that led to this consideration. We're going to get some perspective on this from both NRR and EPRI who have spent an enormous amount of time looking at these tests in some detail to try to understand what their implications are on the behavior of fuel in actual nuclear power plants.

The focus in the presentation of the research program itself, however, is going to evolve for looking at high burn-up fuel under LOCA conditions, and probably maybe even some stuff on ATWS conditions.

With that little bit of introduction, I'm going to turn to the rest of the agenda, and we're going to begin with a presentation by Undine Shoop.

I think most of the members know Undine. She worked with us on some of the steam generator tube rupture stuff. She's going to give us an overview of the NRR Staff's view on the high burn-up issues. Undine, are you ready?

MS. SHOOP: Yes.

MR. WERMIEL: Before Undine, I just have a couple of words to --

CHAIRMAN POWERS: Would you tell us who you are.

MR. WERMIEL: Sure. My name is Jared Wermiel. I'm Chief of the Reactor Systems Branch in NRR. I wanted to just make a couple of introductory remarks and point something out to the Committee that they may not be aware of. When we met with the Staff, the ACRS last May, we agreed to come back and talk about the issues that Dr. Powers already delineated in his remarks.

Today's presentation, as he pointed out, is divided into basically two parts. This morning NRR is going to provide some background and discussion of its current efforts to review new guidance that was provided to us via an EPRI topical report from the industry to justify future burn-ups beyond the current limit of 62 gigawatt days per metric ton.

Undine is going to provide some background, and following her background, EPRI will discuss the topical report itself, and then Undine will give you a little status of where we are with that review at this time.

This afternoon, the Office of Research will update you on their efforts to gather data and address the issues that are identified in the 1998 burn-up fuel program plan.

I'd like to point out that that program plan is somewhat data and we are currently, NRR is currently working with research on an update of that program plan. We hope to complete the update, and put it into the form of a memorandum to the Commission some time by the end of the year, if all goes well. And that's all I had. Undine, if there's no questions, you can proceed.

CHAIRMAN POWERS: Well, I guess a question comes to my mind, a little bit puzzling to me. Maybe none of my business, but I'll ask the question anyway.

MR. WERMIEL: Sure.

CHAIRMAN POWERS: It seems to me I got a notice that said NRR had felt it had no users need for the RES Program, and now you tell me that you're working to help them revise their program plan.

MR. WERMIEL: We view the program plan in maybe a different light than just the matter of identifying user needs, Dr. Powers. We felt the program plan was important because it communicated to the Commission and other interested stakeholders the entire status of the agency's efforts and activities related to fuel.

If there is a user need, we will work out with research exactly what it is. The Office of Nuclear Reactor Regulation needs, by way of the work that research is undertaking. If we don't identify a user need, we still believe it's important that the program plan reflect the current efforts that are ongoing properly.

At this time, I don't know that we've identified a "user need" per se, but we're still discussing this with research, and we haven't made a definitive determination yet.

CHAIRMAN POWERS: Well, it goes without saying that the ACRS proper has been confused by this user need business, and I don't know that we need to go into that.

MR. WERMIEL: We can, if you want.

CHAIRMAN POWERS: Well, I don't want.

MR. WERMIEL: Okay.

CHAIRMAN POWERS: I'd rather get on with the discussion of the technical work right now.

MR. WERMIEL: That's fine.

CHAIRMAN POWERS: Okay. I guess the floor is your's, Undine.

MS. SHOOP: Thank you, Dana. I'd like to talk today about the EPRI topical report on reactivity initiated accidents. First of all, I'd like to go over the history of RIA criteria. That way we can bring everyone up to speed and we're all on the same page for discussing this issue.

Then we're going to have a presentation by EPRI to provide you information about what they are proposing in their own words. And then I'm going to come back and share with you the preliminary review plan of how we plan to address this topical.

RIA criteria history started off back in May, 1972 with Reg. Guide 1.77. This is the original Reg. Guide that had the criteria of 280 calories per gram, and then later in 1993 when the industry wanted to get a higher burn-up. At that time, they were at 30 to 40 gigawatt days per metric ton Uranium, and they wished to go to 60 to 62 gigawatt days per metric ton. And at that time, the Office of Nuclear Reactor Regulation wrote a letter to the Office of Research asking them to evaluate fuel failure thresholds for normal operation and RIA conditions, because we wanted to make sure that as we extended the burn-up, that we had the knowledge to be able to do that type of assessment.

MEMBER LEITCH: I think I missed that number, because I was writing instead of listening. What was the original limit, gigawatt days per metric ton?

MS. SHOOP: Back in 1993, they were at 30 to 40 gigawatt days.

MEMBER LEITCH: Thirty to forty. Okay.

MS. SHOOP: Yeah. And then they wanted to go to 60 to 62.

MEMBER LEITCH: Thank you.

MS. SHOOP: And then in 1997 we wrote a memorandum to the Commission. Basically, we had seen some low enthalpythial bows in the CABRI and NSSR programs, and we were a little bit concerned about it. So one of the things we did is industry came in and they did a generic assessment.

They used a more representative model. They used 3-D analysis rather a current 1-D analysis that's used, to be able to better demonstrate what would actually happen in one of these events. At that time, they showed that with the 3-D analysis they were all well below the 100 calorie per gram limit that had been proposed by research. And because they were under the 280 calorie per gram, and they all demonstrated that they used this more representative analysis that they would meet the lower limit, we determined that they were okay on that basis.

CHAIRMAN POWERS: This always a little bit confuses me. We had a 280 calorie gram limit that became a 225 calorie per gram limit for PWR fuel, and there's a different one for PWR fuel. And that was borne of some tests done a long time ago in a land far, far away.

Then people come in and they say well, we've done these better neutronics, and they say that the power input is much less than that. I have never understood what that has to do with what the limit the fuel will take itself.

MS. SHOOP: Okay. The limit of what the fuel will take it based upon testing criteria that says these are the boundaries at which the fuel can withstand. The more representative analysis that the industry does is an analysis to demonstrate in a real reactor, loaded, with control rod works that are realistic, what will the fuel actually experience?

And what they demonstrated through these analysis is that what the fuel will experience is much lower than the 280 calories.

CHAIRMAN POWERS: And that's fine, and they have to do that. It still has nothing to do with what the criteria are.

MS. SHOOP: Okay. Let me back up.

CHAIRMAN POWERS: Unless you're going to make criteria that's a function of time and impulse shape. Instead, you've got a criteria that's strictly number of calories per gram.

MS. SHOOP: Yes, we do. Okay. So back in 1998, research had provided an information letter, and in that information letter, they proposed changes to the RIA criteria, and they proposed 100 calories per gram. That's what feeds back into our Commission memorandum, that the industry did the representative studies and demonstrate that they could meet that.

WE got together in 1998 between the two offices, and we put together an agency program plan for high burn-up fuels. At this time, the industry mentioned that they would like to go beyond the 60 to 62 gigawatt days per metric ton, and we did an analysis. We determined that with our declining budgets, we would not be able to support all the research needed to be able to do that, so in this agency program plan, we put down that the industry would have to provide the criteria, the database and the models for burn-ups above 62 gigawatt days per metric ton Uranium. That means, in essence, they would have to perform the research to support developing the database to be able to get the information to support extending the burn-ups.

In that agency program plan, we also said that research would still confirm the criteria for burn-ups less than 62 gigawatt days per metric ton, and that feeds back from our user need letter of 1993 when we originally asked them to do that.

The industry responded to our program plan. One of the things that they did was the EPRI Robust Fuels Program, included an objective of being able to develop industry-wide criteria, data, analysis, and models to be able to support the higher burn- up.

This topical report that they're going to present on today is the first topical report that they are presenting that they have given to the agency to be able to address higher burn- up, and to be able to support the criteria development for higher burn-up use.

Their approach is consistent with the current Reg. Guide 1.77 in that it has a coolability limit, and it has a radiological release criteria, so it's still a two-tier approach, which is consistent with our current criteria, and that's what we would be looking at when we review this topical. That's all I have. I'd like to bring on EPRI next.

CHAIRMAN POWERS: Let me ask you just another question about these multi-dimensional kinetics, and I'm quickly getting out of my depth here. It seems to me that in discussing the energy impulses delivered to the fuel by a reactivity event of some sort, a lot of attention has been focused on the differences in the speed with which that energy is delivered to the fuel in reality versus the test.

Now the reality, unfortunately, is a reality that's kind of -- it's an interesting reality. It's not an experimental reality. It's a code calculational reality with these multi- dimensional kinetics models.

On the other hand, I've seen some work at Penn State that says that as the amount of Plutonium in the fuel builds up, that these impulses narrow, and that the calculations that show them remaining wide, are because of some errors in the treatment of the delayed neutrons. Can you comment on any of that?

MS. SHOOP: I have not seen the Penn State reports. I'm not familiar with them. If you could provide a reference to that, I would definitely appreciate it.

CHAIRMAN POWERS: I believe I can.

MS. SHOOP: And with that information, I'd be more than happy to get back to you after I can look at it and intelligently address it.

CHAIRMAN POWERS: I mean, it seems to me you have to look at that because no matter what criteria you say, the licensee is going to have to come in and say well, see, I'm always below that for any hypothesized accident.

MS. SHOOP: Correct.

CHAIRMAN POWERS: And they don't do that by saying see, I've run my reactor and put this impulse into it, and here's the measured data on this. They do this with a calculation.

MEMBER ROSEN: Would you prefer that they run them?

CHAIRMAN POWERS: Well, I would very much prefer to see some experimental data on the impulses in light of the questions that have been raised. I mean, I'm a naive soul here, and a very trusting soul and, you know, these people present me these computer codes where things are calculated out to four or five significant digits, you know. I have great confidence in that until some very smart people from Penn State tell me I shouldn't have confidence in that, and then I'm not sure what I have confidence in.

MS. SHOOP: I think the pulse width may change, but I think that our ability to determine reactor physics and the equations that go into them, and the uncertainties into them are very low. And, therefore, the analysis, as long as you have the right input as far as what the pulse width is, and that's what these tests determine, that the actual analysis is very well defined and well-known.

CHAIRMAN POWERS: Well, of course, that's what the smart people at Penn State are telling me I should be suspicious of.

MS. SHOOP: And that's why I'd like to get those papers, please.

CHAIRMAN POWERS: Okay. I guess we're ready to listen to Rosa Yang.

MS. YANG: My name is Rosa Yang from EPRI. What I'd like to do today, the industry represented by EPRI, the Robust Fuel Program -- there are two parts of the presentation. Like Dr. Powers said, there's tremendous incentive for going to higher burn-up, not only economic incentive but the societal incentive, so this work that will be presented this morning by us is part of our effort in going to higher burn-up.

As I outlined it here, what I'd like to do is to first talk about some of the industry effort related to the topical report that you'll be hearing from Robby Montgomery later on. And he's going to go into the detail, and which may address some of the questions, Dana, that you raised regarding the mechanism of reactivity initiated accident, the impact of pulse widths, temperature, and other stuff.

What I would like to do is to address a couple of the points related to this topical. One of the points I'd like to address is some of the experimental effort, and analytical effort that has been put into this area by the Robust Fuel Program in the industry. And specifically, I'd like to highlight two points raised by this group, particularly the RepNa-1 test. And talk a little bit about the future, which is the CABRI Water Loop Project, to put those two issues into the context related to the submittal of the topical. But I will not address the topical itself, so for the detailed question related to the mechanism and stuff like that, that will be the next presentation. Next slide, please.

Of course, Dana alluded to that the RepNa-1 test from France, which was run in November of 1993. This is the famous test that started it all. It raised a concern about the high burn-up failure limit for reactivity initiated accident may not be conservative enough. And one of the -- let me just get to the test result directly.

The failure limit is 30 calories per gram, as contrasting 170 calories for the failure limit that you'll see later on in Robby's presentation, which is what Undine calls radiological limit, so 30 is much lower than 170. So it raised the question about are we conservative enough? And more importantly, fuel dispersal occurred on this test, so that kind of started the whole thing.

A bit background on that test, and the material is an O-type of cladding, Zircaloy-4, and the burn-up is 64,000. The corrosion thickness on the outside of the cladding is 80 microns, with extensive spallation, the oxide peeling off. The test was run with a very narrow pulse in the sodium loop. Next test.

Tremendous amount of number of tests and effort has gone into in this area to look at this reactivity initiated accident. I just give you some of the effort. This is really just from the experimental side. There's eleven CABRI tests run in France at the CABRI reactor. Thirty-six NSRR tests run in Japan. This number may not seem very large comparing to light water reactor, we have 50,000 rods in one single reactor. However, each of these tests are highly instrumented, and they're fairly expensive. It's on the order of three to five million dollars per test, so these are tremendous amount of effort, and tremendous amount of data being accumulated.

But I think what is more important is not only the data being obtained, but a considerable amount of post-test analyses, and mechanical property measurement, the various laboratories, organizations have been analyzing all these data. And the current situation is, there's a fairly good understanding and agreement what the failure mechanisms are. And in general, most people -- by the way, one thing I want to point out is, NRC has run a PIRT Program, that some of you may be familiar with. And one of the PIRT panel was on RIA, and the conclusion of that PIRT panel was very consistent with what you're going to be hearing later on in terms of the failure mechanism, so I think there's a good understanding of what caused these rods to fail. And, you know, later on you'll see a lot of data which seems seemingly random. But if you consider the cladding ductility of the rods that are being tested, the temperature of the test conditions, the pulse width, you'll see they're actually telling you a very consistent story.

Because of these variables involved that many of the organizations have used analytical tools trying to analyze it, not only to analyze it but trying to link that to the light water reactor condition. The one you're going to hear from us is using FALCON. The French have SCANAIR and NRC have FRAPTRAN.

CHAIRMAN POWERS: You tell me that the data are consistent if we taken into account these factors that you listed down here. I presume there are some others.

MS. YANG: Right.

CHAIRMAN POWERS: But, you know, I have never seen a plot that says okay, your data here are calculations, and notice that they all fall in a 45 degree slope or something like that.

MS. YANG: I think you will see that in our report in terms of predicted versus measured. And you will see some of the -- quite a lot of the data supporting what we're proposed by Robby in a minute.

MEMBER FORD: In your first bullet, the implication is that the RepNa-1 results are, as you said, outliers.

MS. YANG: Right.

MEMBER FORD: They're of no significance. However, of the 47 tests that were done in France and Japan, were any done under exactly the same conditions, Zircaloy-4 oxidized, et cetera, et cetera, to those which were done at RepNa?

MS. YANG: No.

MEMBER FORD: So, in fact --

MS. YANG: There was nothing exactly.

MEMBER FORD: So, in fact, the RepNa results may be relevant. They may not be applicable, but they are relevant. They are relevant data.

MS. YANG: Yes.

MEMBER FORD: It wasn't badly controlled.

MEMBER ROSEN: I think let me help with the question, because I think I have the same sort of question. If you had put a heavily spalled piece of Zircaloy-4 into one of those tests, the 47 tests, which was hit with a nine and a half millisecond is that pulse, would you -- do you think that that rod under those conditions in one of those 47 tests would have failed like in RepNa-1?

MEMBER FORD: That's exactly my point.

MS. YANG: Thank you. I understand the question. Since we -- I'm a scientists. Since we've never done that experiment, I can't tell you what the outcome would be. But based on my judgment, it would not.

MEMBER FORD: Now is that what the --

MS. YANG: And that's why I'm going to give you a little detail on why it wasn't done, and why I think it's an outlier.

MEMBER FORD: But you then go on and say you have some analytical tools.

MS. YANG: Yes.

MEMBER FORD: Would those analytical tools predict the RepNa-1 results?

MS. YANG: No. That's why, if you'll bear with me, that's in my next couple of slides exactly. I'm trying to address your question.

MEMBER FORD: Okay.

MS. YANG: And you're quite right, and I forgot to mention that. I'm probably too nervous. One more thing I forgot to say --

MEMBER ROSEN: Why are you nervous?

MS. YANG: This is an August group.

CHAIRMAN POWERS: These are all sweethearts here. Don't you worry about these guys. They are just -- they're gullible, believe everything that's said.

MS. YANG: You know, I'm very naive, but not that naive. But what I want to say if we have to prepare the presentation, but we have worked in this area since 1994, so we have considerable amount of information on the computer. So, you know, if you don't want to hear any of these, just tell us go through it fast, and then we'll talk about whatever you're interested in. So that's what I meant to say in the beginning, but let me say that now.

So I'm going to tell you why RepNa-1 is so unique. Next slide. Sorry. Let me just sort of finish my thought, and then I'll come back. Because RepNa-1 is so unique, and we formed a RepNa-1 task force to look at all the unique features of it, and that's what I want to spend a few minutes to tell you about. But let me kind of just give you a little bit background about the industry effort in the RIA area in general, not limited to RepNa-1.

There was, as you see, the 1993 RepNa-1 report created all the concerns, and the industry has evaluated all the data, and has created a report that we did not necessarily submit to you, and we did not submit to NRC because there was no regulatory action or licensing activity at that time. However, we did the analysis to ensure ourselves that this is not a concern for the current licensing limit, and we have produced a report, which recognized the core coolability of 230. And if you want to know the difference between 230 and 280, we'll talk about that later. And what is important is, we recognize that there should be a burn-up dependent failure limit, so in --

CHAIRMAN POWERS: Yeah. I have to say that that's something that everybody ought to understand, is that your report recognizes a burn-up dependence.

MS. YANG: Yes.

CHAIRMAN POWERS: Which heretofore has never been recognized in the regulatory process, and that is the biggest take-home lesson I got out of the 1996 report.

MS. YANG: And what we -- at that time, we didn't think we have enough understanding, so we didn't really do too -- although we have analyzed the data extensively, but we didn't use the analytical tool to propose the criteria. What we did was, we kind of proposed a region of success, which is basically bounding all the non-failed data point. Can you go to the next slide? Which is this limit, this dashed line, which is what we call region of success.

I know right now they are not supported by data, but you'll see from Robby's presentation, all the data below here are non-failed. Could we go back? Thank you.

Since that report was issued, several countries have kind of adopted that failure limit, because there's a very conservative approach, supported by the relevant tests. And from 1996 to now, we have gained a considerable knowledge base. As I said, those analytical and experimental, and we have used our code to develop the failure limit, which you will hear later. And we have adopted the no incipient melting to ensure coolability. Next slide.

And I just want to kind of give you the schematic without developing how we -- without really presenting how we developed this, so we have two limits. And as you can see, the analytical developed limit isn't that different from the region of success line that was developed in 1996.

Now let me talk about RepNa-1 now. Next slide, please.

MEMBER BONACA: Could you tell us just one word about FALCON? I mean, what is -- is it a neutronic code, is it three dimensional?

MS. YANG: It is a thermal mechanical fuel performance code. Is it three dimensional? It's probably two dimensional. It addressed the LOCA, in fact, circumferentially. And, of course, the axial dimension, as well.

MEMBER BONACA: So really, it's for purpose of comparing the test with --

MS. YANG: Yes. I'm sorry. I should have said also, is the steady-state in the transient code. The transient part is used to analyze the test and compare the test.

MEMBER BONACA: Thank you.

MS. YANG: And there are quite a few features unique to RIA have been incorporated in the code.

MEMBER LEITCH: Could you define the fuel rod failure, and coolability limits? In other words, what does fuel rod failure look like? What does that mean? Is that a perforation in the fuel?

MS. YANG: It is a breach of the cladding, yes.

MEMBER LEITCH: A breach of the cladding.

MS. YANG: Yeah, that's what failure. And that limit is used to calculate the radiological consequence.

MEMBER LEITCH: Okay. And then the coolability --

MS. YANG: And then the safety limit is the coolability limit.

MEMBER LEITCH: Okay.

MS. YANG: It has to maintain the core geometry.

MEMBER LEITCH: Thank you.

MEMBER FORD: Excuse me, Rosa. I --

MS. YANG: And by the way, Robby is going to talk about that a bit too. I'm sorry.

MEMBER FORD: Okay. Would you mind going back to the previous graph?

MS. YANG: Sure.

MEMBER FORD: I, also, am learning about this. I'm assuming, therefore, that the fuel rod failure --

MS. YANG: Which is this blue line.

MEMBER FORD: That blue line.

MS. YANG: -- and the current limit is the burn-up independent limit of 170 calories per gram, which is saying if 170 calorie per gram was put into fuel, the fuel rod will not fail.

MEMBER FORD: And so the -- any analytical code that you develop for that will have inputs, such as the mechanical properties of the fuel cladding, the degree of hydriding of the fuel cladding. There are parameters in that which take into account.

MS. YANG: Yes.

MEMBER FORD: And the coolability algorithm analysis will have thermo hydraulics criteria.

MS. YANG: Yes.

MEMBER FORD: Heat input criteria into the fuel. Is that right?

MS. YANG: You mean how we developed it?

MEMBER FORD: No. What parameters would be in the algorithm that would define that red line? What sort of parameters?

MS. YANG: How do we define the red line?

MEMBER FORD: No, I'm not interested in -- could you just give me a feeling of the physics. What sort of inputs to the algorithm that define that line? There's an algorithm, an equation that defines that line?

MS. YANG: The current regulatory limit is a straight line 230, burn-up independent straight line.

MEMBER FORD: Okay. So it's defined by policy, isn't it?

MS. YANG: Yes, and some experimental data.

MEMBER FORD: But it's experimental, not analytical. There's not a thermo hydraulic --

MS. YANG: No.

CHAIRMAN POWERS: The upper criterion is one that was invented based on some tests, I guess they started in the 60s actually.

MS. YANG: Yes.

MEMBER FORD: Okay.

CHAIRMAN POWERS: And like sensibly negligible levels of burn-up, imaginative tests, some of them within cladding. It was a long time ago.

MEMBER FORD: Okay.

CHAIRMAN POWERS: Okay? That's really not -- the physics you're looking for really lies in the lower lines.

MEMBER FORD: Okay.

CHAIRMAN POWERS: Not in the upper lines.

MEMBER FORD: Okay. Fine.

MS. YANG: Okay. Now let me address some of your questions about - next slide, please - about

RepNa-1, and what have we done with RepNa-1 is. It's such an outlier or several characteristics. It is a much lower failure limit, enthalpy level comparing to the other RepNa test. Can you go to the next slide?

CHAIRMAN POWERS: In fact, Rosa, correct me if I'm wrong about this, the enthalpy input, integrated input may have been 80, I mean 30 calories per gram, but the failure actually occurred during the power ramp-up, so it actually occurred at even lower enthalpy input.

MS. YANG: Yeah. The total energy input or enthalpy input for this particular test is what, 120 or 110? Something like that.

MR. MONTGOMERY: Robert Montgomery. The answer to that is 100, the energy input is 100.

MS. YANG: Yeah. Right. Thank you. The total energy input is 100. The rod failed at 30 at the peak power location. However, the most intriguing aspect, at least to me as a material-type of person, is the failure did not initiate at the peak power location. In fact, it is very much down below at the rod, and I have a picture to show you in a minute.

Then you ask yourself, what is there that caused the failure? The power level at that location is much lower than 30, maybe something like 26 or 27 or so, so it's not the peak power location. A failure initiated there, according to the organization running the test. And, of course, none of the codes -- you ask can the code explain? The code can explain every other test, except this particular test.

There are other concerns raised about this test. There's a pre-existing defect that was identified after the refabrication. These rods that were tested were from a French power reactor, and they're long, of course. And in order to test it, they cut them short, and then put in end-plugs, and other stuff. And after the refabrication of this particular test, they found an artifact.

CHAIRMAN POWERS: Let's see now. The artifact you're discussing had to do with attaching the ends on this, or was it something that was in the cladding that they cut out?

MS. YANG: In the cladding that were to be tested, not at the end, but at the cladding.

CHAIRMAN POWERS: So it's not an artifact. I mean, it's something that exactly existed in the clad.

MS. YANG: Well, they didn't see it before the refabrication, but they saw it after the refabrication.

CHAIRMAN POWERS: Well, the question is, did they look?

MS. YANG: They did look. According to their report, it was not there. But let me just show you the test. I don't want to make a big deal out of it. I don't think this is the smoking gun, but that's one of the concerns.

CHAIRMAN POWERS: One of the questions that persist in coming up in this is, we say gee, this particular test had spalling clad, it had a pre-existing defect. The question I ask is, well, is that different than the fuel that we would have in the reactors after it had been taken to some elevated level of burn-up? And quite frankly, the databases that I have available for high burn-up fuel never answer that question for me. Some of the fuel seems to be in pretty good shape, but I never get any kind of detail to say over the length of this rod, which can vary from 12 to as much as 14 feet nowadays --

MEMBER ROSEN: In some states.

CHAIRMAN POWERS: -- do we have anything that looks like what you've called here a pre-existing defect? Do we have any evidence of spallation?

MS. YANG: We certainly don't have pre-existing defect. The outcome is that pre-existing defect is a part of the refabrication process, so we don't have that in the reactor. We don't know exactly how those -- I'll show you a picture in a minute. But regarding to the spallation, this is Zircaloy-4 cladding, and when we talk about burn-up extension to 70-75,000, I don't think anybody would use Zircaloy-4 cladding to go there. They're probably mostly looking at advanced alloys, and that's what is pretty much widely used in the industry. So I don't anticipate this kind of material in our burn-up, in our live water reactor high burn-up.

MEMBER ROSEN: Rosa, when you say advanced alloys are you talking about ZIRLO?

MS. YANG: ZIRLO and M5. And as many of you know, corrosion is a temperature driven affect. Some of the low duty plant, they probably could still using the improved Zircaloy-4, which is sometimes called low-tin Zircaloy-4, but it's improved more than just lowering the tin content.

CHAIRMAN POWERS: Of course --

MS. YANG: They're all better than this cladding, is what --

CHAIRMAN POWERS: Well, the problem is it's better on paper. We just don't have any data for reactivity insertion accidents at high burn-up with these improved alloys, do we?

MS. YANG: We will have this year.

CHAIRMAN POWERS: But will and have are two different things.

MS. YANG: Right. I agree. We will have, and they're in the pipe.

MEMBER FORD: Could there not also be a relationship between the pulse geometry as a function of time and the strain rate?

MS. YANG: Yes.

MEMBER FORD: Imposed strain rate. And would not the failure and the clinical failure of Zircaloy-4 change strain rate? Is this not somewhat of an expected result, failure on the forward part of the pulse?

MS. YANG: Yes.

MEMBER FORD: High strain rate pulse.

MS. YANG: It's really not even high strain rate. The whole pulse is very narrow, but at the beginning of the pulse, the rate isn't that high.

MEMBER FORD: No, but where you said it curves, it would be a high strain rate part during the pulse, would it not?

MS. YANG: Not yet. Not at the time of the failure. See, it failed at extremely low power condition.

MEMBER FORD: Okay.

MS. YANG: Let me go on to some of the concerns. Pre- existing defect, instead of going back and forth, I'll show you the picture in a minute. But most importantly, this is the first time 10 millisecond test was run. And when we started looking into the data, we find that, you know, of course the time of failure, the energy input of the failure and all that is dependent on the signals. And they are microphone signals, flow analysis. Bear with me and I'll get into that detail in a minute.

Because the pulse is so narrow and is in the beginning phase, so a very small difference in the uncertainty of the signal interpretation, or the recording time would cause a big difference. And so that's one concern that I'm getting back to.

Another concern was raised by Dr. Hee Chung of Argonne, is talking about this particular rod, because it's a first test. They preconditioned it somewhat differently, at slightly higher temperature, so that could have caused the embrittlement of the cladding. There's another material aspect I'm getting into, so because of all these clouds, if you may, centered around this test, the RepNa-1 task force was formed within the CABRI International Water Loop Project in October 2000.

As you can see, this is kind of a difficult task. On one hand, people outside asking the validity of the test, but you do need the cooperation of the group, the organization conducting the test in order to fully investigate that. I'm personally chairing that group. We have been at this now for two years, and it's a lot of effort, and it's very difficult because we're looking at something that happened ten years ago. Next slide, please.

This is just some table list of RepNa-1 comparing to another sibling test, which is RepNa-10, which is exactly the sibling of RepNa-1. It failed at about 80 calories per gram. And most importantly, there is no fuel dispersal. It failed, but no fuel dispersal. The rods are spalled. The other difference you said has exactly the test been done? No, it was done at 30 milliseconds, because it was recognized that 10 was not representative. Next slide, please.

MEMBER ROSEN: So pardon me, would you go back to that. So I would conclude if those were the only two tests that you had, the big difference was the pulse width.

MS. YANG: Yes.

MEMBER ROSEN: The pulse width at ten milliseconds is simply too much for this fuel. Thirty-one milliseconds is not.

MS. YANG: Yes. Well, there are other narrow pulses done, because one of the speculation, if you may, is the ten millisecond pulse create a gas dynamic loading on the cladding. Thank you. In this one, this particular test was high burn-up, as well, ten milliseconds. The difference is the oxide thickness are different, so it's very good cladding. There are no failures. It goes up all the way to 113 calories per gram, no failures. And one of the reason I list one percent strain is if there's such tremendous dynamic gas loading, you would expect a large strain on the cladding. The result is normal strain, so that's why, you know, I'm not quite convinced about the gas dynamics.

In other tests which were done, unfortunate -- with an even worse cladding spalled, and unfortunately, this one is 75 milliseconds. But again, no fuel dispersal. The rod failed at about the same level as that, so we quite often think these two tests are very similar, and both have no fuel dispersal.

MEMBER LEITCH: But those failure rate, those failure enthalpies are still well below your previous blue curve. Right?

MS. YANG: Yes, because they are spalled, and we -- the proposal that we have does not include spalled rods.

MEMBER LEITCH: I see. Okay.

MEMBER FORD: Can you have pulse widths of the order 10 milliseconds occurring in the reactor?

MS. YANG: No.

MEMBER FORD: It's physically impossible.

CHAIRMAN POWERS: It could, not from a control rod ejection, but I can create a pulse for you, if you want.

MEMBER ROSEN: In a real reactor?

CHAIRMAN POWERS: If you let me borrow the reactor for a while.

MEMBER ROSEN: No, no, no. I'm not going to do that. No, I mean in a real reactor, Dana, is a 10 millisecond pulse at all credible?

CHAIRMAN POWERS: Not for the -- no, not for a natural event.

MEMBER ROSEN: No. So I guess that was the issue.

CHAIRMAN POWERS: I mean, there is this

question that's been raised by Penn State about as you build Plutonium in, the pulses do become narrower.

MEMBER ROSEN: Narrower, but that's a MOX Fuel plant.

CHAIRMAN POWERS: Well --

MEMBER ROSEN: That's a whole nother ball game.

CHAIRMAN POWERS: It's challenging to tell the difference between a MOX Fuel plant, and a high burn-up fuel. You build in quite a lot of Plutonium.

MS. YANG: Well, the particle size --

CHAIRMAN POWERS: Particle size.

MS. YANG: Yeah. So let me say something to you about the RepNa-1 task force. First I want to say, our evaluation is not complete. WE're close, but we're not complete, and so what I'm presenting here is kind of work in progress to show why we did not include it in our evaluation.

CHAIRMAN POWERS: Let me ask you just an opinion here. I mean, you knock yourself out trying to explain one test result, and whatnot, but isn't the really substantive thing that's coming out of all these programs, is that you have a burn-up dependence?

MS. YANG: Yeah.

CHAIRMAN POWERS: And really, that's where we ought to be focusing our attention.

MS. YANG: I agree. I absolutely agree. In fact, you concluded mine for me in saying there is one outlier, and there are so many other good tests, do we really need to really put a lot of effort in --

CHAIRMAN POWERS: I mean, the RepNa-1 is useful for me when I want to badger Ralph Caruso a little bit, but quite frankly, the real issue is, we see a burn-up dependence that we never recognized before.

MS. YANG: And we have a consistent data set, and then we know why they're so consistent. It's really the bottom line I want to leave with you.

MEMBER BONACA: I have a question I'd like to ask you. You showed us a table with comparisons, and we talked about the basis for comparison. On the previous slide, you had a list of concerns regarding RepNa-1.

MS. YANG: Yes.

MEMBER BONACA: Okay. Could you go back to that and tell me how those concerns apply to tests RepNa-5, 8 and 10, versus the number 1?

MS. YANG: Yes.

MEMBER BONACA: Perhaps understanding there is a modifier there, or if you try to -- or if you're addressing the same microstructure, the same conditions and so.

MS. YANG: Yes. In fact, in the report we're going to address all of that. But let me just very quickly -- and again, let me emphasize, we don't have -- we have found several smoking guns. We haven't found the smoking gun. We haven't satisfied ourselves --

MEMBER BONACA: Yeah. I'm trying to understand if we are comparing apples and oranges.

MS. YANG: Okay. This is the first test done, so there's considerable more uncertainty and lack of experience in terms of identifying exactly when the failure occurred. This one, I think they have gained enough experience. All the other are much wider pulse. There's just inherent experimental difficulties in dealing with a very, very narrow pulse like 10 milliseconds.

Now in terms -- this is the only one that we found artifact, and this is the only one that did not fail at the peak power location. All these failed at pretty much near the peak power location.

MEMBER BONACA: The first and second tests, were they -- did they have the same pre-conditioning conditions?

MS. YANG: No. This is the only one that has -- can I go to my next one? That will really answer your question about the pre-conditioning.

MEMBER BONACA: All right.

MS. YANG: Actually, it's the one after that. Can you go to the next slide, please? Maybe just go to the next slide, and let me answer Mario's question.

The artifact, I already talk about it. Go to the next one. I think that's where the picture is. This is where the artifact is. It's like a crater with a depression. This is a crater. There's a depression in it. It's not throughwall. What they did is they found it. They didn't know how it happened. They made an impression of it, and they were able to see the depths of it. There are people arguing, you know, when you make an impression you really don't go deep enough, but that's what was done ten years ago. So this was this artifact, and I'll show you where it is in terms of the rod. This is a real picture.

MEMBER ROSEN: Before you go away from that, can we look at it together for just a second more. The artifact -- to me, there are two artifacts there. There's a scratch also.

MS. YANG: Oh, the scratch. Oh, that is -- rods have scratches. That's not --

MEMBER ROSEN: Yeah, but rods have scratches because when you put the rod into the grid --

MS. YANG: Yeah, exactly.

MEMBER ROSEN: -- they scratch.

MS. YANG: Yeah, you should ignore -- I don't think this is that significant, because most rods have scratches.

MEMBER ROSEN: Have scratches. Okay.

MS. YANG: Yeah.

MEMBER FORD: But you don't think that when you do the pulse there's -- that is the -- that could be the defect --

MS. YANG: That's what we -- let me kind of --

MEMBER ROSEN: I want to understand Peter's point.

MS. YANG: That's a speculation at this point.

MEMBER ROSEN: Peter, did you just say that you think it's possible that the defect that caused the failure is the scratch, not the crater?

MS. YANG: Oh, the scratch? No, no, no.

The scratch is very shallow, and all the rods have scratches, and the scratches pretty much run along the rod.

MEMBER FORD: From that rather shallow delve, can't be very high.

MS. YANG: No. Oh, you mean the --

MEMBER FORD: Yes.

MEMBER ROSEN: From the scratch.

MEMBER FORD: The value for that must be very small.

MS. YANG: Yes.

MEMBER FORD: That is, even if you have a shallow scratch, sharp scratch, which that looks like, and it's a long scratch.

MS. YANG: Yes.

MEMBER FORD: Then during the heat-up, the pulse, then the high strain rate condition -- I'm hypothesizing these things --

MS. YANG: Yeah.

MEMBER FORD: During the high strain rate, a portion of the pulse, during the pulse width you could exceed K1C, G1C for that.

MS. YANG: I don't think so, because all the other rods have scratches.

MEMBER FORD: Okay.

MS. YANG: I would -- all the rods have these scratches because when you pull the rods, you always have the scratches, and they're very, very shallow.

MEMBER FORD: Okay.

MS. YANG: This is the artifact, and if you -- let me, since I'm on the artifact, let me go to the next slide. The artifact is here. The peak power location is about here. The artifact is here, and the IRSN, the organization running the test said that the failure occurred about here. Okay? And this is a peak power location. There is where they think the failure occurred. This is where the artifact is. And by the way, this is a schematic of how the rod looked like after the test. You have tremendous amount of material lost. This is the, you know, the loop, so that's just to give you a sense about what the -- roughly what the location is like, if you can go back to the last slide. One more.

There's an artifact. I showed you that, and I'm not sure. I'm not saying that's a smoking gun. I'm not sure. WE're evaluating it, because there are very -- they took a lot of cut after the test, but they couldn't find it. But the rod was so badly cracked as a result of the test, so it's hard.

Another thing is that they didn't make a good indication of the azimuthal orientation, so they don't know where to look for it, azimuthally. They know roughly where to look axially, but they didn't know how to look -- so the artifact was not found. So that's one of the concerns that we're chasing.

The other concern we're chasing is the pre-conditioning of RepNa-1. Because it's the first test, and Hee Chung has a hypothesis that because this particular test was done at higher temperature, 380 comparing to 310 for 14 hours, and all the RepNa tests were conditioned at lower temperature for a slightly shorter time, so he hypothesized it may have embrittled the cladding. And we're evaluating that, and I don't want to talk yes or no on that hypothesis, because we're in the middle of the evaluation. And it's so controversial, and I'm not done with our task force.

And we're also comparing, as I said, we think the RepNa-8 and 10, although they were somewhat different pulse widths, but they are sibling rods, they are spalled, and we're looking at the ductibility of the cladding and the failure mode, so that's on the microstructure, which is one part of the investigation. The other part, which I think is equally important, is when the rod fail, if you can go back, I think just one slide, which is on the signal analysis, which is really even more interesting that we found quite a few things. You know, these are highly instrumented tests, as I said earlier.

There's microphone, which is basically used to indicate when the failure occurred. They had microphone from the top and bottom based on the different --

MEMBER ROSEN: What are they listening for?

MS. YANG: The sound.

MEMBER ROSEN: Yeah, I know. The sound of what?

MS. YANG: The sound of -- that's exactly a relevant point. The sound of failure, they think.

MEMBER ROSEN: What does it sound like?

CHAIRMAN POWERS: Crack.

MEMBER ROSEN: But you have a test. Is there flow going through this rod?

MS. YANG: Yes.

MEMBER ROSEN: There's flowing liquid metal, actually.

MS. YANG: Right.

MEMBER ROSEN: And so it makes some -- you have a background noise.

MS. YANG: Right.

MEMBER ROSEN: And you sit there, and you listen, and you hear shhh. Right?

MS. YANG: Yeah.

MEMBER ROSEN: And then you do this test, and you hear something different.

MS. YANG: Right. You're absolutely right.

MEMBER ROSEN: What is it you're hearing?

MS. YANG: It's whatever you're hearing, and the expert -- you know, that's why in this one, I'm relying a lot on experts who are familiar with the signal to interpret it, because there are a lot of noise involved, and have to sort of find the relevant signal.

CHAIRMAN POWERS: You're listening to the propagation of a crack.

MS. YANG: Yeah.

MEMBER FORD: A ping.

CHAIRMAN POWERS: Yeah.

MS. YANG: I'm going to tell you, not just the crack would make the sound. The crack initiation could make sound. The oxide cracking could make sound. In fact, we have actual experience that shows the sound come from other stuff, as well.

MS. YANG: Okay. So they look at different -- they also have flow meters that look at flow change as a result of rod failure. Sorry. The expansion of the cladding, and after the failure there are material dispersed, so that changed the flow, and the pressure sensor. So they have all these recorded. And, of course, the organization running the test are the expert in interpreting these.

The very low value is based on the microphone signal. And exactly answer your question, does microphone only listen to failure, or it could listen to others? In fact, there was a test that they heard three microphone signals, and after a lot of analyses and all that, they concluded that some of this microphone signal they heard earlier was not failure indication, but rather maybe oxide cracking, or whatever. So they actually, they themself did not rely 100 percent on the microphone signal.

Another, to me, maybe even more disturbing situation which shows uncertainty is the flow meter signal and the pressure sensor. The flow meter, we're dealing with 1cc difference in the flow, and --

MEMBER ROSEN: One cc per second, per what?

MS. YANG: One cc total difference between the flow meter from the top and the bottom, as a result of fuel change -- fuel rod change in the dimensional.

MEMBER ROSEN: Flow is typically in terms of a mass flow rate, or a volume flow rate, not a

MS. YANG: It is, yeah.

MEMBER ROSEN: What do you mean when you say a cc, a cubic centimeter without a time?

MS. YANG: Well, the flow will change once the -- it will change as a result of fuel expansion, and it will change after the rod fail.

MEMBER ROSEN: Well, it changes, I agree, and flow rate -- you're saying the flow rate changes, because the flow channel is obstructed. I agree with that.

MS. YANG: Yeah.

MEMBER ROSEN: But when you say 1cc, I don't know you mean. Is it 1cc per second, 1cc per minute, 1cc per hour? The flow rate change, I'm trying to get a sense of --

MS. YANG: It's been a while since I looked at it.

MEMBER ROSEN: -- how big the flow rate change was.

MS. YANG: Do you know what is the --

CHAIRMAN POWERS: Can you tell me what flow rate we're talking about?

MEMBER ROSEN: Flow through the --

MS. YANG: It's the flow rate of the sodium in the channel of the --

MEMBER BONACA: Actually, the delta would give you the flow rates.

MS. YANG: Yeah. It's the delta.

MEMBER ROSEN: You put this rod in the channel and you establish flow. You know what it is. And then when you fail a rod, the flow changes. Typically, it goes down. Pressure goes - - Delta P goes up, the flow rate goes down. And you say 1cc. I say okay, 1cc per what?

CHAIRMAN POWERS: No, I think it's just a volume change that you have.

MEMBER ROSEN: Well, why don't we -- Rob, do you know the answer to that question?

MR. MONTGOMERY: I think I can help you answer that question. The 1cc that Rosa's referring to is at the instant of failure indicated by the flow meters. The difference in the inlet flow meter and the exit flow meter was 1cc at the time of failure.

MS. YANG: But they'd still have a unit

though. Is that per second?

MR. MONTGOMERY: Well, it's integrated -- it's at a particular point in time. Yeah, the fuel rod expanded at that particular point in time.

CHAIRMAN POWERS: And you had a volume displacement.

MR. MONTGOMERY: And basically, at that point in time, it displaced 1cc of sodium, as determined by the difference in the inlet flow meter and the exit flow meter.

MEMBER ROSEN: So essentially, instantaneous.

MR. MONTGOMERY: Instantaneous.

MS. YANG: Yeah.

MR. MONTGOMERY: At the point of --

MS. YANG: Basically, you're looking at very small differences, because what you are looking at is when the failure occurred that makes enough of a difference in the flow rate, and since the magnitude is so small, that it's hard to compare with another point. And a new point was, they have different recording systems. You know, they have three different recording systems to record the time zero for the flow meter, for the flow rate. And the different recording systems give you somewhat of a conflicting time. And during this two years we've been back from A system is the best, to B, and back to A, and then back to B, so we've been flip-flopping quite a bit.

In one of those systems, that would give you a value which is like 60 or 70 calories per gram, very similar to RepNa-8 or 10. And the other would confirm that it should be about 30, so because of all these conflicting things, and we've been flopping back and forth during the two years of our investigation, and the difficulty is, it has been -- most of the data were just stacked in the drawers during all this time. And most of the people running the experiment were not there, so we're not sure we'll ever get to the bottom in terms of signal analyses, because it's so complex, and then we're not sure we have all of the data available.

So at the last meeting, we kind of just throw up our hands and say we've done this enough. Let's call it quits. Instead of arguing is it 30, is it 50, is it 60, let's draw a range saying that's the uncertainty of the test. Kind of what Dana said, hey, do we -- how much effort do we want to spend on a single test that may not be representative. So if you go --

MEMBER ROSEN: So you have victory is what you're saying. You declared victory.

MS. YANG: Well, I'm a scientists, Steve. I'm trying to get to the truth.

MEMBER ROSEN: Well, not through the -- you're a scientist, and I grant that. And you've been trying to get truth, and I grant that. But you're not trying to get to the truth through RepNa-1. And it's not necessary that you get to the truth through RepNa-1.

MS. YANG: I'm glad to hear that, but there's always people ask what about RepNa-1? So that's why we've gone through this trying to --

MEMBER ROSEN: The industry has supported a tremendous amount of effort to try to understand RepNa-1, and what you've concluded is that RepNa-1 probably demonstrates a failure for all these conflicting reasons, between 30 and 50 calories.

MS. YANG: Right. Right.

MEMBER ROSEN: Good enough.

MS. YANG: And we just want to put it in proper perspective for all the -- but I want to say is during this whole exercise, we have a much better understanding of how to record the signals better, to interpret the signal better. We have a much better understanding about the microstructure different among the various tests which were the data were there, but because of this exercise, we have a much better understanding of the failure mechanism, I believe.

MEMBER FORD: You didn't say too much, or I didn't hear you say too much about the microstructure. Was it hydrided?

MS. YANG: It was.

MEMBER FORD: You mentioned the oxide thickness, but presumably that relates to hydriding?

MS. YANG: If you would allow me just to escape that, because that's the most sensitive issue right now, and there's just tremendous debate about it. I would rather not say it until we come to the conclusion. There's significant hydride on the material, so that's kind of where I think all of you pretty much already concluded for me that the RepNa-1 is probably not a representative test. And it is okay not to include it in this analysis. And more importantly, we are going to M5, ZIRLO low- tin cladding for those conditions.

MEMBER ROSEN: But I won't let you escape that slide without talking about the bottom line. Typical PWR pulse is around 30 milliseconds.

MS. YANG: Right.

MEMBER ROSEN: What do you mean? Is that typical in a reactor?

MS. YANG: No. I mean, obvious -- thank God, we never have a rod ejection rod drop accident. Typical in the licensing framework.

MEMBER ROSEN: In the licensing framework.

MS. YANG: With conservative licensing calculation, typically -- I mean, we have some maybe 20, 25, but typical range.

MEMBER ROSEN: People who do calculations in support of licensing of these kinds of fuel assemblies use a pulse that's about 30 milliseconds, even though they know there's really no way to get to that fast a pulse in the real reactor.

MS. YANG: Yes. Thank you, Steve. Thank you for pointing that out. That's exactly the truth. You really have to stack up conservative assumptions in order to get a pulse. That's why it's called licensing calculation. And because of that, and this is kind of an agreement among the various group, and I'm not saying it's unanimous, but most of the CABRI test has been run at this pulse width, and from now on will be pretty much run at that pulse width.

Now if you could -- I'm going to direct my to some recent industry effort related to supporting the topical, my next slide. I know I'm not supposed to be here talking to you about the Robust Fuel Program, but that's something near and dear to my heart, so I have to say a few words about it.

The Robust Fuel Program, RFP is what we call it, was formed in 1998, and some of the people in the room actually as a champion for forming this program. It's really a utility initiative trying to keep our fuel safe and economically operating. Operating economically is -- here are some of the objectives that we're driving at, is no operational surprises. We want fuel to perform as advertised. No regulatory surprises, because right now we have some of these surprises, so we want to get rid of those surprises. And that's why we're proactively supporting the RIA evaluation, which is an important aspect of the focus of the Robust Fuel Program.

And after we kind of address our current problems, our interest is in burn-up extension. Here's a little cartoon that was drawn for our program.

CHAIRMAN POWERS: Rosa, let me ask a question. I know you're not -- we didn't give you any time to talk about this Robust Fuel Program, but I'm willing to bet that the Subcommittee and even the

ACRS as a whole, would be very interested in your program. When would be an appropriate time for you to come talk to us about this program, or maybe would you please keep in mind that we'd like to hear about the program, and suggest to us a time when you know.

MS. YANG: Be happy to. Any time.

CHAIRMAN POWERS: Any time.

MS. YANG: Yeah.

MEMBER ROSEN: This I think, Rosa, just for the benefit of some of the Subcommittee Members who may not know about it, is a very expensive program that has gone on for many years. It's the utilities' money. Well, like I think it was like --

MS. YANG: It's all utility money. Right now it's about $10 million per year.

MEMBER ROSEN: Per year. And it's been going on for how many years now?

MS. YANG: Since 1998, about four, five years.

MEMBER ROSEN: So it's $50 million already been spent on this. It's not a small thing, so I think the Committee would be interested in it.

MS. YANG: And it's worth every penny of it.

CHAIRMAN POWERS: Well, I think -- I mean, I think that our interest would be most peaked when they get to the burn-up extension portion of it. Clearly, operational surprises and regulatory surprises are of interest, but I think the burn-up extension is probably where we're most interested in it.

MEMBER ROSEN: Some of the operational and regulatory surprises have been cured, like with sticking rods, that sort of thing.

CHAIRMAN POWERS: Sure. Sure. Yeah, I think we ought to try to interact with Rosa, and find a time when she can come talk to us about this, get an idea of whether we should do it Subcommittee-wise or Full Committee, because I'm sure the Full Committee would be interested. Maybe some time after the first of the year.

MS. YANG: Sure, that's good.

MEMBER FORD: Rosa, could I ask also the question. In the planning for this program, you obviously had in mind the current light water reactor fleet. Is there any part of this plan that takes into account advanced light water reactors?

MS. YANG: No, but from every document -- no, because from every document I read about advanced light water reactor, they usually just say they use the fuel at the time, so there's, you know -- not really that I see, a lot of work that goes into different fuel.

MEMBER FORD: There's no different.

MR. SIEBER: No, light water reactor is light water reactor.

MEMBER FORD: But do the advanced light water reactors, part of the strategy is to go for extended burn-up periods.

MR. SIEBER: Then you need research like this to do that.

MS. YANG: Yeah.

MEMBER FORD: But there's no difference than if you go to MOX fuels, no change?

MR. SIEBER: Yes, there is.

MS. YANG: MOX will be different. The program was formed by the U.S. Utilities, as you know, in the U.S. Only Duke Power is interested in MOX, so this program has not addressed MOX.

MR. SIEBER: Other than particle size, all fuel becomes MOX fuel, so you're going to learn about it anyway. I do have a question though. All these tests were run with sodium as a coolant. Right? And so you have to take into account when you apply that light water reactors, the difference in the cooling fluid.

MS. YANG: Yes.

MR. SIEBER: How is that done, other than to say well, we know, you know, what the heat transfer is and flow rates, but you don't know the interaction between the sodium and the clad, and obviously, velocities are different. And, you know, there's a lot of impacts there, and maybe you could say a couple of words about that.

MS. YANG: I'll say a couple of words, but if it could wait until Robby's presentation.

MR. SIEBER: Fine.

MS. YANG: We believe that sodium tests are relevant and conservative, because the sodium apparently are more efficient in conducting the heat away than water, so it would keep the cladding temperature cooler. And in terms of cladding mechanical property at lower temperature, the cladding is more brittle.

MR. SIEBER: Right.

MS. YANG: So we think the tests are relevant and conservative. Next slide, please.

For burn-up extension, as Undine alluded to earlier, that NRC has mandated that the industry does the work for the burn-up extension. The industry proposed a consistent set of criteria, proposed data to develop the criteria, and to demonstrate the compliance. So with that mandate, there are three major focus. The Robust Fuel Program focus on full burn-up extension.

The first one is industry guide, which is the framework for burn-up extension, is to say what type of criteria are needed, what type of data are needed for burn-up extension. The RIA which is culminated in the work of the topical that will be presented later. The LOCA, and I think Ralph probably will talk some of the joint effort in the LOCA area. And this is a little bit of a commercial for just saying, you know, the Robust Fuel Program is not just off-set type condition type of thing. We do do a lot of work that confirms the steady-state operation, high duty fuel designs, but the same set of data are the basis for burn-up extension, so the type of work we do are poolside inspection at the power plants, hot cell examinations, laboratory tests, laboratory testing included both in test reactors in the laboratories to provide the data. Next slide, please.

Let me just give you a quick sense about the type of poolside and laboratory tests - sorry, poolside and hot cell. I'm not going to talk about laboratory tests at all today.

The BWRs we have two campaigns, one at 57,000 which is below the current licensing limit. The other is for burn-up extension at 70,000, and noble metal chemical addition is the current practice for BWRs, and we will compare the impact of that on fuel performance.

For the PWRs, we look at two advanced alloys, both at 70 or a little bit above 70, 000 burn-up, and we'll be looking at fuel properties, cladding properties, and all the other stuff.

MEMBER ROSEN: Now help me understand, Rosa, how these plants got to these very high burn-ups. I thought 62 was the limit.

MS. YANG: Yes, these are LTAs.

MEMBER ROSEN: Lead Test Assemblies.

MS. YANG: Lead Test Assemblies.

MEMBER ROSEN: Where you're allowed to go beyond the limit --

MS. YANG: Yes.

MEMBER ROSEN: -- for a few rods.

MS. YANG: Right.

MEMBER ROSEN: Okay.

MR. SIEBER: Well, actually the whole assembly.

MS. YANG: For fuel assembly. Right. Thank you. Of course, these rods, some of them -- especially the Limerick rods are currently in the Argonne hot cell for the LOCA test. Next slide, please.

I'm running out of time, so I'm going to run through very quickly about the CABRI Water Loop Project, because --

CHAIRMAN POWERS: Rosa, let me worry about the time. You worry about making sure the Committee understands.

MS. YANG: Okay. Because Robby really has a very good presentation.

CHAIRMAN POWERS: Fine. You let me -- I will worry about the time, and you guys worry about presenting understandable materials.

MS. YANG: All right. For the RIA, we have submitted the topical, and that's the purpose of the presentation later. We have -- another effort is the CABRI International Water Loop Project. This project, by the way, is a $62 million project. It will run 12 tests, so that gives you a sense about the magnitude of this type of test. And, of course, they will be run. The difference here is they want to run it in a prototypical water loop under the PWR conditions.

Some of the special feature of that test is they will run advanced alloys, which I think this is the most interesting to the Robust Fuel Program. They will run two tests in 2002, one M5, one ZIRLO. They will run tests with very high burn-up fuel, about 70 or 80. They will show the fuel coolant interaction because this is water, so you can get the fuel cooling interaction after the rod failed.

They will also run tests to show some mechanistic understanding of the mechanisms, in fact, the pulse width, grain structure or whatever. And the reason I say whatever is because some of the tests are not clearly defined at this moment, and which is appropriate.

MEMBER ROSEN: Now, Rosa, are they on schedule to get all this done in 2002, which is fast coming to an end?

MS. YANG: Sorry. Only two tests are run. Next slide, please, then you'll see. Only two tests, which is what we call CIP. CIP means CABRI International Project, and they have six series. And two of the tests will be run this year, which is a little bit behind schedule. It was supposed to --

MEMBER ROSEN: In the sodium loop.

MS. YANG: In the sodium loop. And then they are going to do the -- you see there's a -- I'm not good at using the pointer. You see there is a three year gap here. That's when they're going to take out the sodium loop, convert to the water loop. And then they're going to run a qualification test to make sure thing go well, and then they're going to run tests in the water loop in 2006, to sort of parallel the test run in sodium to sort of bridge the gap.

MEMBER ROSEN: To really answer Jack's question about, you know, what's the difference between sodium and water?

MS. YANG: You'll see that comparison in 2006. And to answer your question

CHAIRMAN POWERS: Mark your calendar.

MEMBER ROSEN: For four years.

MS. YANG: Okay. So they're going to run some high burn-up tests. They already talk about mechanistic understanding, MOX fuel to be defined. So that's coming. Next slide, please.

The two tests that's most interesting to the industry are these what we call CIP-0 Tests. They will be run, one in October, in this month. In fact, the 17th of October, and the other will be run next month. The first one will be run is this advanced alloy called M5, which is used mostly in France, but now in the U.S., as well. This particular cladding, the oxide has always been low, about 20 micron, and you can see at such high burn-up.

CHAIRMAN POWERS: When you have very thin oxides on the M5 clad, do you pick up a lot of hydrogen in the --

MS. YANG: No. In fact, the characteristic of the M5 is the hydrogen pickup fraction is lower than Zircaloy-4, so they not only have low corrosion, they have low hydrogen pickup. These are from literature, and we have -- the hot cell program will confirm that in our program later on.

CHAIRMAN POWERS: It seems to me that I saw a report from Canada on its Calandria tubes which are made out of M5, reporting some, not all, but some of those tubes show an elevate level of Deuterium pickup. Do we understand that?

MS. YANG: I'm not familiar with that, Dana. If you could tell me more about it. Based on what --

MS. SHOOP: Actually, Dana --

MS. YANG: Sorry?

MS. SHOOP: Could I interject something in here? Framatone has recently shared with us some plots of the M5 hydrogen pickup versus the Zircaloy hydrogen pickup, so we'll have to share them with you to show what their results have been.

CHAIRMAN POWERS: I mean, what I could derive from this report from the Canadians was that many of their tubes -- they went to the M5 to reduce the Deuterium pickup. And on a few of their tubes, they saw an anomalously high Deuterium pickup and, of course, you know, what I was seeing was a report on the theory of why something should have an anomalously high Deuterium pickup. And quite frankly, it didn't persuade me, but I'm not that smart, so maybe other people know things about this.

MS. SHOOP: We'll have Framatome address that, but they have shown us the plots of that.

CHAIRMAN POWERS: Uh-huh.

MS. YANG: Okay. So the test will be performed in a week or so, and it will be done with 30 millisecond pulse. And the energy that can be injected is 95 calories per gram, because that's the highest they can put in for such high burn-up rods with this facility. You know, the new facility will be better, but for this, that's what we get.

For the ZIRLO rod, this particular ZIRLO rod is from Spain. It has very high corrosion. What

I listed here is the maximum corrosion of the rod, but the test section will be a little bit lower, at 85.

CHAIRMAN POWERS: Let's say an important thing to understand better, when you quote these oxide layer thicknesses, do you have a feeling for what the uncertainty is in those? And the reason I ask is, I see things in your topical reports correlating things against oxide thickness, and Least Squares Fits against oxide thickness. And yet, where the oxide thickness is taking a precisely known value, and whatever they're correlating against is assumed to have some scatter in it. Whereas, it seems to me that both the dependent and independent variable have a substantial amount of scatter. And that ordinary Least Square Fitting is not the appropriate technique.

MS. YANG: Yes. Robby have slides that will show the sensitivity as a result of the uncertainty. And let just address your questions about uncertainty. Yes, the uncertainty of these measurements are, I would say about 10 to 20 micron also, maybe 10 micron is what it would be. And another thing to point out is these are the maximum thickness of the whole rode, as there's azimuthal variation, and there's tremendous axial variation.

When we do the RIA test, we usually pick the top section for a couple of reasons. One, this is the most brittle section because of the highest oxide thickness in the reactor, and the other is for the PWR rod ejection, the energy is dumped mostly in the upper portion of the rod.

CHAIRMAN POWERS: One of the phenomena we've seen is that as people go to high burn-up fuel, of course, is a tendency for some deposition of Boric Acid on the upper sections of the rods. I noticed that you had test plans in which you're going to look at what this noble metal chemistry did to the surface of the rod. Are you also going to look at what this Boric Acid absorption, or have we gotten rid of that by going to the M5 cladding?

MS. YANG: Oh, boy. You have several questions. First, let me answer yes, we are looking at Boric Acid deposition on the upper portion of the PWR rod, which we refer to this anomaly as axial offset anomaly. Now that from our current understanding is the result of CRUD deposition on the upper span of the fuel rods. M5 is better in terms of corrosion between the cladding material and the coolant, so if the duty of M5 is high enough, I think we would have similar problems, like the CRUD deposition and the resulting --

CHAIRMAN POWERS: CRUD comes from the piping system, not from the clad.

MS. YANG: Yes, from steam generators, pipes, so that the corrosion in terms of oxide may be low, but the CRUD is still there.

MR. SIEBER: I think CRUD deposition is a cycle phenomenon, rather than a life-time phenomenon, because of what you do when you shutdown, is to borate the system heavily, which loosens a lot of CRUD, which you then remove, and so you go through these peaks and valleys in operational --

MS. YANG: We get rid of a lot of the CRUD that way, but those we don't get rid of in our program, we also developed a technique to clean it.

MR. SIEBER: Right.

MS. YANG: To ultrasonically clean off the CRUD.

MEMBER ROSEN: Which, by the way, you should show the Committee when you return next year.

MS. YANG: Okay. Is one of the reason we spend $10 million a year. Okay.

MEMBER ROSEN: Pretty neat.

MS. YANG: Pretty neat. Right. Where am I? So this ZIRLO have 100 micron very high burn-up, and the test will be performed a month from now, again at 30 milliseconds with about the same energy level. There's not a big difference between M5 and ZIRLO. It's whatever maximum you can get.

Now there a couple of new parameters involved in these two tests. The most important one is the first time we test advanced alloy. Dana, you asked about that. Yes, we will confirm this test for advanced alloy, is the higher burn-up than our current experience database from 63-73,000 burn-up.

So let me conclude my short presentation with, we submitted the topical, and I think, you know, there are tremendous databases supporting this submittal. There are over 80 RIA simulation tests using irradiator rods rather than unirradiated rods. And more importantly, we have a very large corrosion database, and couple that with the mechanical property test, because Robby will outline for you, it's not the burn-up, but rather the condition of the cladding that determines if the rod will fail, or not. And he'll also show you some analysis and experiments on fuel coolant interaction.

Now the test to be performed later this year, in fact, this month and next month, will just confirm the conservatism in the proposed criteria. And if the fuel suppliers want to use those data to develop higher values for the advanced alloys, they can do that. But in our point of view, we just want to use that to confirm the conservatism in our proposed criteria.

We do not think we need the water loop in order to draw conclusions from the RIA topical, because as I answered one of the questions earlier, the sodium test results are very conservative, because you have lower cladding temperature. And, you know, we already have 80 some good tests, another six, another half a dozen because some of them are in sodium, some of them are comparison. Another six tests is not really going to change the picture.

Now one of the concerns is DNB. What about DNB-induced failures? I made some broad statements saying they're not expected at this proposed value. I know that's a broad statement, and Robby is going to address that, because that's part of our entire submittal. So if you have any questions, I'll answer them. Otherwise, I think we should turn to the --

MEMBER LEITCH: I have one question. I guess you -- I'm coming away with the conclusion that RepNa-8 and 10 are still considered to be valid tests. But if I go back to your curve of enthalpy versus burn-up, the colored curve, if I plot that --

MS. YANG: They're below.

MEMBER LEITCH: They're well below.

MS. YANG: Yes.

MEMBER LEITCH: The blue curve, for example.

MS. YANG: Yes.

MEMBER LEITCH: And I don't understand why that is the case.

MS. YANG: Okay.

MEMBER LEITCH: Why wouldn't the blue curve be done through the RepNa data?

MS. YANG: Let me give you a short answer, and Robby will give you a long answer.

MEMBER LEITCH: Okay.

MS. YANG: The simple answer is, those two rods are heavily spalled. And the criteria that we have developed is for high burn-up, and we do not think we will use spalled rods for high burn-up. So in our database we clearly separate those rods that have spalled, and those rods that have not. So the criteria that we proposed are not for spalled rods, so your observation is quite correct. They are below the curve, and he'll show you that we show the mechanical property of spalled rods, are considerably worse --

MEMBER LEITCH: But in the operating reactor, there are some spalled rods.

MS. YANG: Right now, yes, but not as we go to advanced alloys. Yes, you're quite right. Some of the rods have spalled, but is very small number of rods, and we are talking about a very local phenomenon here.

MEMBER LEITCH: Okay.

CHAIRMAN POWERS: Are there other questions for Rosa? Rosa, I have a question on your proposed test matrix for the CIP Program. I don't think your slide intended to lay out a detailed test matrix, would indicate just the general types of test. But one of the things that I know about tests of this nature is, if I could do exactly the same test twice, I would not get the same answer, because there are -- though you might try to control a lot of the variables that affect the rest results, it's physically impossible to control them all.

Do you plan in that program to have a test in which you attempt to measure the magnitude of the experimental layer, essentially doing the same test twice? And if not, why not?

MS. YANG: Dana, let me first say it's not my test matrix.

CHAIRMAN POWERS: I understand.

MS. YANG: It's a test matrix proposed by IRSN, the French safety authority who will run the test, and it's being discussed and debated among all the participants, and we are just one of them.

MEMBER ROSEN: Which includes the agency.

MS. YANG: Which includes the agency. In fact, they and EDF funding the major share, the lion's share. Two-third of the program are funded by the French, so they're a little bit more equal than the rest of us.

MEMBER ROSEN: But there's U.S. government money, particularly from the NRC in this.

MS. YANG: Yes.

MEMBER ROSEN: And there's utility money, as well.

MS. YANG: Yes. So we do have a seat at the table, and we do try to argue as strongly as we can, but we're just one of the participants. Among others are the Germans, the Spanish --

CHAIRMAN POWERS: Regardless of the nationalities involved, understanding the magnitude of experimental error seems to me a critical factor.

MS. YANG: Yes, I agree with you. And that very issue has been debated a lot within the program. And we will continue the deliberation of this, but most people do not really want to spend $5 million, or $3 million, whatever the number is, just to duplicate the test. They think a lot of the experimental uncertainties could be gleaned from others. And if you look at - - one thing, Dana, I would agree with that a little bit. I mean, there's always a lot to be said about duplicating exactly the same experiment. But if you look at the whole data set, run at such vast different conditions, they're very consistent.

CHAIRMAN POWERS: I would be intrigued to hear a statistician justify that position.

MS. YANG: Okay.

MEMBER ROSEN: These are wealthy statisticians. Very wealthy statisticians.

CHAIRMAN POWERS: Well, quite frankly, I have taken the position, I think I am willing to defend the position that when you have a few expensive tests, it's more critical than ever to measure the experimental error.

MS. YANG: You can --

CHAIRMAN POWERS: If I have a lot of easy tests to do, I can get away with not measuring the experimental error. If I have only a few and they're very expensive, I should focus on measuring the experimental error.

MS. YANG: I think you are right, Dana. And like I said, we can discuss and debate that within the CABRI Water Loop. What I want to point out is, maybe it will be very clear from Robby's. At the end of his presentation, we are not using these tests in a statistical sense to develop the criteria. We're trying to understand the basic mechanism of reactivity-initiated accident, and how the failure occur. With that understanding, then we look at how consistent the data are, so the understanding is eventually benchmarked by these simulation tests. So these simulation tests give us a lot of information, because it's not just a go/no-go. It give you the emission gas release, it give you the strain on the cladding, it give you, you know, some of the microstructures, so you really have a wealth of data coming from a single test. I think, you know, it is -- they should not be treated in a statistical sense. I think --

CHAIRMAN POWERS: The problem is that you get all these data, and you do not understand how much of the variability that you see is a function of uncontrolled parameters in the test. And I guarantee there are some.

MS. YANG: Uh-huh.

CHAIRMAN POWERS: And without having that understanding, you can be fitting noise, you can missing the most important affect, you can end up spending millions of dollars for finding a code to account for an anomaly in the experiment, where you would be knocking yourself out on understanding something like oh, maybe RepNa-1.

MS. YANG: Yes, it's possible. I think the RepNa-1 Task Force investigation have produced quite a lot of some of this uncertainty information you're talking about, and I briefly mentioned some of those in terms of timing, in terms of the magnitude. So I'm not trying to disagree with you. I'm just mainly pointing out some of the considerations that has been discussed during the CABRI Water Loop Project.

CHAIRMAN POWERS: Yeah. Quite frankly, I hear it on all expensive test programs. I heard the same stories, and I will reiterate --

MS. YANG: That's one of your frustrations. I understand.

CHAIRMAN POWERS: Well, you have this, literally a hundred years of people understanding how to design experiments efficiently and whatnot, consistently coming back and saying you have to measure the experimental layer, and for some reason, we blow that all off, and say we will neglect a hundred years of people saying here's how to design efficient experimental programs, and not measure experimental layer because it's too expensive. And quite frankly, it's too expensive not to measure the experimental layer.

MS. YANG: I agree. Just for you maybe a little bit comfort is CIP0, and CIP0-1 are, in a way, kind of a duplicated test, if you ignore the coolant conditions, which I think is reasonable to ignore. But they are sibling rods, and they'll be duplicated.

CHAIRMAN POWERS: Good. Any other questions for Rosa? I propose that we go ahead and take a break here for 15 minutes. Unless there are people with airplane connection problems, I'll be kind of easy on when we end, and I'll let it run until we're done and whatnot.

MS. YANG: Okay.

CHAIRMAN POWERS: Okay. Let's take a break until 25 of the hour.

(Whereupon, the proceedings went off the record at 10:19 a.m., and resumed at 10:38 a.m.)

CHAIRMAN POWERS: We're going to now have another presentation that Rosa has set put for us with Robbie Montgomery. He's going to walk us through some technical bases here. Robbie has, of course, appeared before the Committee before. He takes the heat so that Joe Rashid doesn't.

(Laughter.)

Joe's gotten chicken or wise in his old age, I'm not sure which.

(Laughter.)

The floor is yours, sir. And, again, let me worry about the time, you go ahead and worry about communicating well.

MR. MONTGOMERY: Okay. Thank you. Thank you. I'd like to thank everyone for letting me come talk today. As Rosa mentioned, I'll be talking about the technical bases that were used to support the fuel failure and the core coolability acceptance criteria that she presented in the previous presentation.

Just a brief outline, I'll just familiarize everybody with the regulatory bases for the reactivity accident. Typically, that would be a control rod ejection accident from a hot-zero power or hot-full power bed. Then I'll go over some discussion about the database of the RIA simulation tests. Rosa alluded to a few of those tests, and I'll go through and show you some of the characteristics of the test and some of the test conditions and try to familiarize everybody with the terminology of what we talk about when we discuss RIA tests. And then I'll go through a discussion of the technical bases that we've used to establish the fuel rod failure threshold.

I'll go through some of the cladding failure mechanisms, both at low burnup and high burnup. I'll talk a little bit about the development of the cladding failure model that we've used to understand and interpret the experiments and then discuss the revisions that we're proposing with regards to the failure threshold limit used for those calculations. And then I'll go on into the safety limit and core coolability limit, talk about some of the issues related to that, how high burnup fuel influences those issues and then discuss the methodology and the revised limit for the core coolability. And then, finally, I'll try to go through a short summary of what I've said.

So it's a lot of material, but I'll try to move through it. Please, as you guys have done already, you're going to ask me lots of questions, I'm sure.

The regulatory background, Undine mentioned briefly the background. Here we have the two limits or the two criteria. One is the coolability limit in red there. It's been defined in the Reg Guide 1.77 as 280 calories per gram, and that's a radially averaged fuel enthalpy, and I'll get to what that means in a minute. It's basically set up to address the GDC, the General Design Criteria, 28. Typically, nowadays, most people use a lower value in their licensing submittals, so generally around 200 to 230 are the values that are used.

Cladding failure threshold is used for meeting dose requirements -- radiation release requirements. It's defined in a number of different places, SRP 4.2 for BWRs and Reg Guide 1.77 for PWRs, and it has a number of values or parameters are used to define fuel rod failure. For BWRs, 170 calories per gram radially averaged fuel enthalpy used. For BWRs and hot-full power BWR events -- PWRs, I'm sorry, PWRs and hot-full power BWR events, DNB is typically used to define fuel rod failure. At this point in time, in the current regulatory base, they're burnup independent, so that's how they're shown here.

CHAIRMAN BONACA: Just one point I would like to make.

MR. MONTGOMERY: Sure.

CHAIRMAN BONACA: You mentioned that typically they submit that like 230 calories per gram. I think one of the reasons, however, is that they use very conservative methods which have been approved 20 years ago and because the limit is going anyway, they don't want to invest money. I mean they also neutronics calculations that show much lower values. They simply don't want to license those codes for economic reasons oftentimes, and so the documents show very much higher limits. I'm just mentioning this because we saw certain data down in the 100 range and below, then we see the values in the FSAR 280 and we think there is such a disparity. I don't think there is that much a disparity, okay? When they do calculate this peak clad temperature with the neutronics codes, three dimensional codes, the get much lower results.

MR. MONTGOMERY: Certainly. Certainly, that's correct.

CHAIRMAN BONACA: They don't need to document them in the FSAR because they were documented a long time ago and they're still below 280. So just to precise that.

MR. MONTGOMERY: Thank you. Now, when we

look at the database here, I'm plotting a reduced set of the database. This is primarily all the data that has been tested for radiated material. As was talked about this morning, there's a large database of unirradiated tests that have been done. I've included a half a dozen or so at the zero burnup line, but there's actually hundreds of rods at the zero burnup line, I didn't include them all. What I've shown here in the database is the 80 or so tests that have been done on rods or rodlets that have been pre-irradiated in either a commercial reactor for a good number of these or in some sort of test facility, the SPERT facility, for example -- not SPERT, but the CDC, the driver core, for example. Some of those have been irradiated there. Some of them have been radiated in a Japanese test reactor called the JMTR reactor.

You have -- okay, so I've indicated here which test programs they come from. NSRR would be the Japanese program, CABRI would be the French program, you've heard something about that this morning already, PBF, the Power Birth Facility at Idaho, and then the older CDC SPERT tests. And I've only included a small sampling of those tests.

What I'm showing here is the radially averaged peak fuel enthalpy versus the segment burnup for the segment that was tested. These tests range from six-inch tests. Most of these are six-inch segments, six to eight inches. That would be the square NSRR program typically uses a six-inch section. The CDC program is about the same, about a six-inch section. Those are indicated in red. The CABRI program typically use 50 centimeters, so you'll have to do the math in your head about how long that is, about a foot and a half. Here is the CABRI program primarily.

You see a generally downward trend with the data, but that's indicative typically of the fact that these test facilities can only put so much energy into the rod or reactivity into the rod. And as a consequence, with burnup increasing, the reactivity of each rod generally drops. So the downward trend is indicative of how hard the test facility can test those particular samples.

Interspersed here, there are solid symbols. The solid symbols indicate that those are tests that had cladding failure during the pulse or following the pulse in each of these. So you see that there are some failures interspersed amongst some of the ones that did not fail, the survivors we call them. This tells us that burnup is probably not the parameter to correlate this data against, because we see that there is no clear separation between the failures and the non-fail tests.

So let me just briefly just show you a comparison, and I should point out too that in this database there's a variety of pulse widths. They vary from as low as four milliseconds to as high as 70 milliseconds. They are a variety of coolant temperatures and conditions. There's stagnant ambient water at 25 degrees C, and there's flowing sodium at 280, 290 degrees C. There's flowing water in some of these tests. The PBF were in flowing water, 1000 Psi, approximately 280, 250 degrees C. So you have quite a bit of mixture in there and the type of test conditions as well.

So here's just an example of a RIA-type pulse. We have a nine-millisecond pulse here, typical of a CABRI-type test. You have a 40-millisecond pulse, more consistent, say, with a typical PWR rod ejection event. And then even some wider pulses. And it's showing you the magnitude. And the area under the curve, the amount of deposited energy for each of these pulses is the same.

MEMBER ROSEN: And, again, a 40-millisecond is not a true in-plant event --

MR. MONTGOMERY: Correct.

MEMBER ROSEN: -- it's a value that's chosen to represent conservatively an in-plant event.

MR. MONTGOMERY: Yes. Just briefly, a schematic to show some of the terminology that I will refer to and have already referred to today. We have three curves on this plot. Again, I'm plotting time along the X axis and then power or energy or enthalpy along the Y. The pulse is here. Typically, what we mean by the pulse width is the full width at half the maximum value. Not all the pulses are Gaussian-shaped in the experiment. Some of them are double-humped, some of them have some nuances. So when you hear someone give a range of a pulse width, for example, RepNa-8, it has a pulse width range between 65 and 75 milliseconds, it's because it's a little difficult to define exactly where the full width half max is for a double-humped pulse.

The consequence of this pulse is an energy deposition, and that's this curve here which gives us the energy deposition as a function of time. And it's just simply the integration of the area under the power time curve. And typically we refer to this in terms of calorie per gram as well. So you may hear terminology like the test experience 100 calories per gram deposited energy. So that would be a value out here. The maximum deposited energy, that would be the integrated energy of the power time curve.

And then you have the enthalpy curve. That would be the solid curve here. And this is the response of the energy deposition. And this is a integration of the temperature, stored energy in the fuel as a function of time. And typically we call it radially averaged, so we're taking the average across the radius of the stored energy.

MS. SIEBER: The downward slope at the end, I take it, indicates the fuel is being cooled?

MR. MONTGOMERY: Correct, correct. So, generally, you have a maximum radially averaged fuel enthalpy that occurs during the power pulse or shortly thereafter, because depending on the width of the pulse heat conduction effects can begin to drive it downward.

The fuel enthalpy may start out at a non-zero value depending on the test conditions. For tests done at room temperature, the enthalpy's essentially zero, the initial enthalpy. And then at elevated temperatures, say in the CABRI facility where you're at 280 degrees C or at a hot-zero power state, you have some initial enthalpy which is typically on the order of 15 to 17 calories per gram. So let's see, we've talked primarily about that.

We generally look at the tests in terms of their radially averaged fuel enthalpy, and so the database that I was referring to here this is the radially averaged peak fuel enthalpy, and it's been determined by a number of different methods. Some of them take into account the heat conduction effects, some of them do not. So in and amongst this data, there is some uncertainty with regard to the fuel enthalpy when you first look at it. Okay.

Here, as a result of an analysis for one of the RIA experiments, what I wanted to illustrated here just to give an example of the fuel temperature profile across the pellet at different points in time during a power pulse. So what I have shown here is the fuel temperature as a function of radial position. And this is for a burnup of 65,000 and a pulse width of 9.5 milliseconds. And I've indicated here the range, the pellet is given here out to just a little over four millimeters. And then the cladding is this outer half millimeter range. At the early part -- in the early part of the pulse, during the upsweep, when there hasn't been very much energy deposition, you see a fairly cool central part of the pellet, and because of the radial peaking due to the plutonium build-in at the pellet periphery, you'll see there's a temperature peaking region here in the pellet periphery. At that point in time, the cladding really doesn't know what's going on yet. It's still sitting there very innocently minding its own business.

And then later on in the pulse, near the peak power, typically, depending on the pulse width, you'll reach the maximum temperature, and that will occur out near the pellet surface, generally 100 to 200 microns inside the pellet surface because of heat conduction effects. And then cladding now begins to feel some of the heat as heat conduction begins to move some energy from the fuel into the cladding.

And then as the pulse progresses, heat conduction begins to become more dominant, and then approximately two to three seconds after the pulse is over, you'll then develop -- the fuel will then develop a more characteristic parabolic temperature distribution that we're all familiar with, and the cladding is now heated up.

So as I said, the test database that we have on reactivity accident tests is pretty much summarized here on this table. We have a variety of different initial temperatures, different types of coolant conditions, different types of pressure conditions, they're pretty similar, though, quite a variety of pulse widths and a variety of energy depositions. In the early SPERT programs, they tested up near 350, 400 calories per gram. The more current programs have really focused on ranges more like less than 200.

Comparing that to light water reactor conditions, there's some differences, there's some similarities, but in all there's enough differences that it really is difficult to apply the data coming from these test programs directly to a light water reactor. So there's a need for using analytical tools to assess the test results, interpret them and then compare back and translate them back to LWR conditions.

MEMBER ROSEN: Well, hold on just a second. that 25 to 90 in the RI column is what your estimate is of the real pulse width in a reactor now?

MR. MONTGOMERY: Again, these would be based on --

MEMBER ROSEN: If you have a full rod ejection.

MR. MONTGOMERY: -- full rod ejection, licensing-type analyses where you've made conservative assumptions on the parameters of control rod worth. This would be the range of pulses that you would expect to see.

MEMBER ROSEN: So the 40 you saw before, the 40-millisecond pulse you saw before you said was not typical of a LWR. Did you say that because of the 90 value?

MR. MONTGOMERY: No. I said it would be typical.

MEMBER ROSEN: Oh, you did. I misunderstand.

MR. MONTGOMERY: I'm sorry, I must have misspoke then. Yes, the 40-millisecond pulse that I showed in the previous slide would be representative of -- this pulse here would be representative -- in the range of a licensing-based --

MEMBER ROSEN: Of what could really happen if in a PWR a rod was fully ejected.

MR. MONTGOMERY: Right. That's correct.

MS. YANG: No, no. The best estimate we did not get a pulse. That's a conservative licensing calculation, as Robbie said several times. The 40 millisecond we call representative is representative in the licensing calculation, but you are asking question about if you have a rod ejection in a PWR.

The best estimate does not show any pulse. The best estimate doesn't show a pulse, but you have to use conservative assumptions in order to get a pulse, because we're dealing --

MEMBER ROSEN: Why does it show no pulse if the rod is ejected? Is it so slow?

MS. YANG: Yes.

MEMBER ROSEN: If you actually had a rod ejected, it would be so slow that there wouldn't be a pulse, you're saying.

MR. WERMIEL: We'll talk about this some this afternoon, so -- we could talk it about now, but let Ralph, when he comes up this afternoon, say some more about this.

CHAIRMAN BONACA: Just a question. From any conditions? Those are from, for example, have zero power? I mean we assume all rods inserted and you're pulling out one? I mean I would expect to see an effect there.

MR. MONTGOMERY: Well, there is an effect but it generally is not a prompt event. You have to have -- I'm not a neutronics expert so I'll try not to get too -- I'm going to get in over my head real quick -- but it's the addition of all the -- assumption of all the parameters that go into calculating a rod worth that gives you the prompt event. And it difficult to -- unless you assume very conservative values for things like neutron lifetime, Doppler coefficients and all the parameters that go into rod worth, it's difficult to make it a prompt event. You'll get an event, you'll get generally a fast rise to power, but you won't have a prompt pulse. It will go to some power level very fast, but you won't have a pulse because it won't be prompt, you'll be less than a dollar.

MS. SIEBER: And you don't have damage in short-term unless you have a prompt event.

MR. MONTGOMERY: Yes. The prompt event then gives you -- obviously, it gives you the rapid rise in the fuel enthalpy because you get this, in effect, an adiabatic type of energy deposition. It needs to be on the order of less than a second to deposit energy faster than the fuel conducted out.

MEMBER ROSEN: I'll wait for later, but I think I'm beginning to understand. We'll hear more about it later.

CHAIRMAN BONACA: Yes. Except that this goes counter to a lot of physics calculations. So it will be interesting to hear more about that there isn't any pulse.

MR. MONTGOMERY: But given a licensing-based approach where the assumptions that go into the calculation of rod worth used in a multi-dimensional physics calculation would generally give you pulse widths that are in this range, and it really depends on the rod worth and these sorts of things.

Now, what have we learned from this database? What we've learned is that the cladding failure response -- I'm going to talk initially about cladding failure, then I'll come back and talk about coolability and fuel rod geometry effects and that discussion. So with regard to cladding failure mechanisms, what we've learned from the database is that there are essentially two failure processes or mechanisms that are active in a fuel rod during a reactivity accident.

The first one generally occurs at low burnup, and that's a high temperature failure response caused by post-DNB operation, and when you go into post-DNB operation you get the cladding temperature excursion which initiates oxidation effects and possibly ballooning effects, and that is generally what happens at low burnup. At low burnup, the pellet cladding gap is generally fairly wide, and the cladding ductility is good. And it can survive any sort of pellet cladding mechanical interaction that goes on at low burnup. But once you get into post-DNB operation there's potential for cladding failure due to the oxidation processes or ballooning type processes.

At high burnup, where now we have -- the gaps tend to have closed or become quite small and the effects of oxidation and hydriding and irradiation damage have all combined together to decrease the cladding ductility, then the failure process is transitioned from a high temperature response to, I don't want to use the word "low temperature," but cooler temperature response where the cladding hasn't seen much heating to failure by cladding ductility processes.

CHAIRMAN POWERS: Let me ask you a question, Robbie. On one of the previous slides, you showed the database, and in that database you quoted the pressure at which the tests were run. And all the tests were at relatively modest pressures with fuel rods that had been reconstituted, yet the accidents of interest are at high pressure. And whereas we probably don't worry about the pressure effect when we're on the left-hand side of this current plot, the low burnup side, it seems to me that pressure becomes a concern when you're on the high side where your failure is due to pellet clad mechanical interactions. why don't we worry about the pressure at which these tests are run?

MR. MONTGOMERY: The primary effect of temperature is the pressure differential, and in the experiments that the pressure differential is set up through the re-fabrication process, and generally the pressure is equal to or less than the external pressure in the experiments that have been done on pre-irradiated material. There have been tests done where the pressure differential is positive and looked at the ballooning effects. At high burnup, we don't expect rod pressure to be a real dominant mechanism because the pressure differential is negative still at hot-zero power, because the fuel is a bit cooler and we license generally to pressure levels that are equal to system pressure at power conditions. So the pressure differential is negative, if you will, it's coming from the outside instead from the inside.

And then, secondly, at elevated burnup, the axial gas communication is quite restricted because of the closed gap and the tight condition between the fuel and the cladding. So the pressure, which is generally -- a majority of the gas is resident in the plenum doesn't have the time in the time frame that we're talking about, less than a second, to migrate to these regions and to contribute to any additional PCMI loading. I'm not sure if that answers your question, but those are the --

MS. SIEBER: I'd like to ask a question that would display my ignorance. If in a practical reactor with a best estimate calculation you can't achieve reactivity insertion that would give you a prompt pulse, then why don't we concentrate on making sure that the mechanics of reactivity insertion will not provide a prompt pulse rather than do all these experiments on what happens to the clad after you get one?

MR. MONTGOMERY: That's a good question. Unfortunately, I don't have an answer for you.

MS. SIEBER: Is this a political question?

MR. MONTGOMERY: Are there any more questions regarding this?

(Laughter.)

MEMBER ROSEN: You mean there's no one in this room who would venture an answer to Jack's question?

MR. ROSENTHAL: Rosenthal. I'm the Branch Chief of the Safety Margins and Systems Analysis

Branch, and we have discussed that at the conclusion of all of this really the free variable is the core design since the rod patterns and the rods are fixed in an existing reactor and that one could design such that you limit the rod worths, and then the rod worths, in turn, determine the pulse widths and, in turn, the enthalpy deposition. So that when you're all said and done, from a very practical reload standpoint where you have to do analysis every 18 months, you might come up with a surrogate in terms of rod worth that ripples through. So we have had those discussions, but I think at this point we're trying to still understand the underlying phenomenology. But, yes, you're right, pragmatically that's where you may end up.

MS. SIEBER: Well, I'm listening to discussions on how much all this costs. On the other hand, part of the solution to this gets back to Dana's comment of an hour ago, which says you ought to really know the experimental and calculational uncertainties to be able to really put your arms around what's going on and what's important and what is not important from a practical phenomena standpoint. And, you know, I'm all for learning everything about everything, and you can make a career out of that, but, you know, once you can establish that an event is precluded, then that changes the focus of where you want to spend your resources, I would think.

MR. MEYER: Ralph Meyer from Research. I think the practical answer to the question is that in the past licensing calculations have been predominantly done with point kinetics models --

MS. SIEBER: Right.

MR. MEYER: -- which are grossly conservative and they give big numbers.

MS. SIEBER: Yes, they do.

MR. MEYER: And so they give energy depositions, fuel enthalpies that are in the range of 100 or more calories per gram. Now, everybody now has --

MS. SIEBER: And they're fictitious, right?

MR. MEYER: -- 3-D kinetics models and nobody has -- well, the models have been submitted, but as far as I know we are not routinely reviewing results of those to the point where we could address this issue. I know at least in the context of this generic issue that the industry has not come forward with 3-D calculations that could be reviewed by NRC that say we're way out of the ballpark on this subject.

CHAIRMAN BONACA: And the reason is that you've kept the limit at 280. I can tell you for a fact, being from the other side for a long time and being involved in this. And the reason is that there is no motivation for a vendor to come in and modify its methodology and have it qualified and accepted, modified and validated, when they can still use the point kinetics combined with a PDQ peak 2 average and can stay well below 280. So what's the point? I mean some of the analysis on the dockets go back to 1968, '70.

MEMBER ROSEN: If George Apostolakis were here, he would go right through the ceiling because he would say it's exactly the same reason that licensees don't do better PRAs. There are no real requirements.

CHAIRMAN BONACA: Well, but I think it's important to understand that from the perspective of the vendors and the owners they are aware that the results are much less severe than what is in the FSAR. You just simply don't go in and change an FSAR if it is a bounding value that is still there. I mean how many of those values in the FSAR go back to 1970?

CHAIRMAN POWERS: I mean I think what you're seeing is a statement on the state-of-the-art that preceded 1983 --

CHAIRMAN BONACA: That's right.

CHAIRMAN POWERS: -- that a high licensing criteria was set that could be easily met with conservative analysis methods. The general belief of all concerned, regulator and licensee, was that nothing would ever approach that in a conceivable core design. There was no incentive to change the criteria, there was no incentive to improve the analysis. What upset that was in fact the RepNA-1 test.

CHAIRMAN BONACA: Absolutely.

CHAIRMAN POWERS: And we should all hail RepNA-1 for having awakened us to the fact that fuel is important and whatnot and let it go at that and move on.

(Laughter.)

I will comment that we're spending most of this morning dealing with RIAs, and certainly that was where this thing started. This afternoon, we're going to deal with other aspects of high burnup fuel, LOCA, ATWS, things like that, which are also important. With that, I'll give it back to you, Robbie.

CHAIRMAN BONACA: One last note I would like to make then is that this is an example of where because of those licensing constraints, maybe we have failed to learn something here that has imposed enormous conservatism and maybe enormous regulatory burden, but the industry has accepted it in place of itself, because we didn't go forward, we understand these issues. If in fact you can convince me that you're not going to have any pulse resulting from a rejection from any conditions, then I can tell you how many places there are where those kind of previous commitments are a burden to the utility.

MEMBER ROSEN: Well, beyond burden, Mario, which I agree with, what concerns me about this in a very general and broad sense is that it diverts attention from the really risk-significant accidents that could occur and their enthalpy deposition parameters.

CHAIRMAN POWERS: It's one of the fundamental flaws of the design basis accident concept, which you and I have decried for advanced reactors.

MR. MONTGOMERY: Okay. Well, back to the cladding failure processes that we were talking about before. Effectively, there are two processes. Just to remind everybody, we have a low burnup -- a process that's primarily active at low burnup and that's the post-DNB response due to high temperature mechanisms, such as oxidation, induced embrittlement or ballooning response. And then this typically occurs after the power pulse when energy's had time to conduct from the pellet to the cladding and initiate the post-DNB heat transfer processes. And then as burnup proceeds and we changes induced in the rod as a consequence of burnup, either through -- well, both through pellet cladding gap closure and changes in material ductility. it's possible to induce failure for a PCMI, pellet cladding mechanical interaction, process during the power pulse. If in fact it's possible to survive in some way, either through improved material ductility, the power pulses at high burnup -- then the post-DNB operation could become effective or active.

So just to reiterate a few points. Cladding mechanical failure mechanism is PCMI resulting from the pellet expansion and fission product matrix swelling in the pellet. The controlling factor or the key factor is the material ductility, the cladding ductility. This conclusion is consistent with the PWR PIRT that was done a couple years ago, a year and a half ago.

The burnup is not really a key factor. It does influence the gap closure processes and initiating of PCMI, but it's really the field duty that drives the corrosion and hydriding process that define the residual ductility. We know that spalled rods, which we've talked about briefly and I'll talk a little bit more, has significantly less ductility than the non-spalled rods. And we see that at high burnup, for rods that have no spallation, no oxide spallation, but still high, on the order of 80 to 100 microns but without any spallation, they have not failed up to now.

MEMBER ROSEN: Can you zero in on that for me that last statement, that spalled rods have significantly less ductility than non-spalled rods. Spallation is a surface phenomena on the outside of the rod -- of the oxide layers on the outside of the rod surface. The ductility is a property of the remaining un-oxided, non-oxided cladding.

MR. MONTGOMERY: Correct.

MEMBER ROSEN: So how are these tracks connected?

MR. MONTGOMERY: How are they connected? That's a very good question. During the oxidation process, certain fraction of the hydrogen is produced due to the chemical reaction. It's absorbed into the cladding and is resident in the Zircaloy matrix material. If the cladding oxide is rather uniform, then the temperature distribution generally azimuthally and axially is rather uniform, and the hydrogen stays rather uniformly distributed. There's some gradience through the thickness that occur because of the temperature grading across the thickness of the clouding. But azimuthally and axially, the hydrogen stays rather uniform.

Once spallation happens, and the spallation process is the local loss of oxide cracking and falling off the oxide layer, you get local perturbations in the cladding wall temperature. Either they're hot because there is an insulating layer of oxide and steam that's ingressed in a crack between the oxide layer before it's fallen off. You might have a local hot spot. Once the oxide has fallen off and exposed either bare metal or a thinner oxide, maybe it's gone from 100 microns to ten microns, then you have a cold spot. These local temperature variations induce thermal gradients that drive hydrogen to move and become non-uniformly distributed. And you get localized areas where hydrogen concentration is elevated. That can increase to pure zirconium hydride levels and be on the order of several thousand ppm locally. And this hydrogen is what influences the material ductility. And it's the non-uniform distribution of the zirconium hydrides that have the biggest impact on the material ductility.

MEMBER ROSEN: So once a piece of oxide spalls, it cools off the cladding in that region and hydrogen moves into this cooler region of the cladding?

MR. MONTGOMERY: Correct.

MEMBER ROSEN: Creating lower ductility in that region.

CHAIRMAN POWERS: What you're making an argument is that you get the hydride precipitation following a spalling event. I could have gone through the same argument and said that it's the hydride nodule that causes the spalling event. And I mean the argument would go along something like this: That when I look at a detailed stress/strain analysis of the oxide growth process, I find that the compressive stress in the oxide imposes a tensile stress on the underlying metal. And that as long as that metal is ductile, everything is fine. As soon as it embrittles, then I get a separation at the interface causing the spallation event. That loss of ductility could come from the formation of a hydride.

MR. MONTGOMERY: Well, I haven't really gone into the details of exactly what drives the spallation process. The spallation process is very complex process. It obviously is one process that could lead to the spallation. But we have seen from micrographs of non-spalled material with very thick oxides, 80 to 100 microns, generally the hydrogen is rather uniformly distributed around the azimuthal dimension. There is generally a gradient through the thickness. There's local deposition -- or precipitation of hydrides near the outer surface of the cladding due to the thermal grading and stress grading that you point out. These have an effect on the ductility but not a dramatic effect as what arises from spalled material.

The spallation process where the oxide falls off and creates cold and hot spots is what leads to the non-uniform hydride distributions. Local hydride, sometimes we use the word "lenses" or "blisters" to define a region of maybe three or four clad thicknesses in azimuthal angle, a few degrees, ten- to 15-degree angle, where you have a very high concentration of hydride. This results from the spallation process and generally is not observed when you have a uniform hot side.

CHAIRMAN POWERS: Well, I mean it's a question of cause and effect. I mean the problem, of course, is that you only see after the spallation event where a spallation has occurred. But it's not obvious to me that you can immediately conclude that the hydride precipitation that you see there followed the spallation event and didn't precede it.

MR. MONTGOMERY: Well, yes, we don't always see exactly what has caused the spallation event. We do see end rods that have spalling. There are regions that don't have spalling because it's a very local phenomenon. So the micrographs are available a few inches above or a few inches below where you have a uniform oxide layer and you see these fairly uniform hydrogen distributions, but when you move up into the spalled region, then you see these non-uniform hydride distributions. You're correct, we don't know --

CHAIRMAN POWERS: I will argue that in every case where we've seen a spall and looked at the underlying material, there's something unusual down there. And that something unusual could have led to the hydride formation and the hydride led to the spalling rather than the spalling leading to the hydride.

MS. YANG: I think whatever the cost -- well, we don't know. Actually, we don't know --

CHAIRMAN POWERS: You're going to have to be on the record or we'll never know what bit of wisdom you gave us.

MS. YANG: Oh, no, I wouldn't go that far.

CHAIRMAN POWERS: Well, you can't talk unless you're on the record.

(Laughter.)

MS. YANG: I think the mechanism is not very important here. There are different -- it could be hydride to drive the corrosion --

CHAIRMAN POWERS: Oh, Rosa, let us have some fun discussing science instead of all this practicality stuff.

(Laughter.)

MS. YANG: Okay. In that case, we can debate the mechanism. What I want to point out is when you have spallation you have hydride lenses form depending upon the degree of spallation, and sometimes the lens could be very thick into the cladding. What I was drawing on the picture is what Robbie just said, that in the right-hand side which is a regular PWR rod that you have some hydride on the cooler part of the cladding and that's a normal condition. When you have spalled rods -- it needs the spalled rods and we don't know which, chicken first or egg first, but you have these spallation, you have these hydride lenses and that's what really causes the cladding to behave quite differently. And he'll show you some mechanical property data that clearly shows the two types of cladding behave rather differently.

CHAIRMAN POWERS: Well, see, the difficulty is this: That one could come along and say, okay, we can take this fuel up to high burnups as long as you don't see any spallation in the course of going up there, because that will lead to hydrides. Well, if the hydrides come first, then that criterion is no good anymore.

MR. MONTGOMERY: Okay.

MEMBER FORD: Robbie, does barrier fuel cladding come into the equation, this disconnect between non-barrier fuel cladding and barrier fuel cladding?

MR. MONTGOMERY: Barrier fuel cladding, if you're referring to the type of fuel cladding that's used in BWRs --

MEMBER FORD: Correct.

MR. MONTGOMERY: -- the oxidation response in BRWs is generally considerably less than PWRs.

MEMBER FORD: No, no. I was really driving at the fact that cladding ductility is a key determining factor.

MR. MONTGOMERY: Yes.

MEMBER FORD: If you have zirconium barrier on the ID of the tube, then that must affect the overall mechanicals in plants.

MR. MONTGOMERY: It does some.

MEMBER FORD: It does.

MR. MONTGOMERY: I mean that's generally included -- when we measure mechanical properties of barrier cladding, it's inherent in that database because we generally don't separate that out. We don't separate the barrier. When cladding with a barrier is tested for the mechanical properties, it's tested as a unit. The barrier is included. And so whatever effect the barrier has on the material properties is inherent to that data. Do you understand what I'm saying?

MEMBER FORD: Correct. We'll bring it up as you go on.

MR. MONTGOMERY: Yes.

MEMBER FORD: Because if you want to use a barrier fuel cladding, then you could well not have any mechanical failure because of the interaction between the --

MR. MONTGOMERY: Oh, I see what you're saying now.

MEMBER FORD: If the barrier fuel cladding came out because of PCMI problem.

MR. MONTGOMERY: Right. And what we're talking about here is not really stress corrosion cracking induced failure, this is really a bulk material response. So the PCMI that I'm referring to here is really being controlled by the entire cladding wall thickness and not the inner surface. The barrier liner was set up to limit localized stress effects and other things, which --

MEMBER FORD: No, I wasn't really talking about ID as being the final failure mode.

MR. MONTGOMERY: Right.

MEMBER FORD: I was talking about the zirconium barrier is purely a compliant layer between the fuel, expanding fuel, the fission gas, and the relatively unductile Zircaloy-2 in this case. But the same principle should apply to Zircaloy-4 because it wasn't compliant there. I take it that hasn't been done. There hasn't been done the same tests on Zircaloy-2 as has been on Zircaloy-4.

MR. MONTGOMERY: No. There have been some

RIA tests on Zircaloy-2 material with barrier material.

MEMBER FORD: Oh, there has.

MR. MONTGOMERY: Yes, there has.

MEMBER FORD: Okay.

MR. MONTGOMERY: In order to understand the high burnup cladding failure process, we needed to develop a cladding failure model, so a cladding failure model based on PCMI conditions is what I'm going to talk about next. And the model is based on strain energy density concept or parameter.

We looked at the -- generally, when a mechanical property test is done, you get parameters such as stress and strain, yield stress, ultimate tensile stress, uniform elongation and total elongation type parameters. If one integrates the stress/strain curve from the mechanical property test, you end up with a strain energy parameter, called the strain energy density. And, generally, that's the critical strain energy density if you carry that integration out to the point of failure in the mechanical property test where you're measuring things like yield stress and ultimate tensile stress. We call that the critical strain energy density.

The strain energy density is just simply the integration of the stress/strain response. What we're talking about here, in the analysis of a reactivity initiated accident test, an RIA test, a code such as FALCON, it was referred to earlier, a field performance code that would calculate that response would calculate the stress and strain evolution in the cladding, and that would be what we call the SED. This concept or approach addresses the effects of strain rate brought up earlier, temperature and the stress condition by axiality, tri-axiality stress conditions. And it's a measure of the loading intensity on the cladding.

The CSED, which we determine from mechanical property tests, it brings in the material characteristics such as the hydrogen content, the temperature, the hydrogen morphology and distribution, and it is used as the parameter to define the point of failure. The cladding is calculated to fail an analysis -- if the SED from the response of the fuel during the power pulse exceeds the CSED, then it would be --

CHAIRMAN POWERS: Robbie, I guess I don't understand how your strain energy density takes into account the strain rate.

MR. MONTGOMERY: Because here in the

calculated strain energy density, you're calculating the response of the cladding as a consequence of the energy deposition. So the response of the cladding is going to become a function of how fast the energy is deposited in the fuel.

CHAIRMAN POWERS: And it's because of the way that you're going to incorporate the properties of the cladding into the calculation.

MR. MONTGOMERY: Yes. And also in the CSED material database, these mechanical property tests are tested with certain types of strain rates. So the constuitive law that you have here that drives the stress/strain law incorporates it as well.

MEMBER FORD: But the CSED will also get some sort of strain rate.

MR. MONTGOMERY: It could be, yes, it could be. The database that we have so far that I was just about to show has a range of strain rates in there. Now, in analyzing in this data, we didn't find a strong dependency of strain rate in this database. This is a database of medium to high burnup fuel cladding properties that we had available to us to use to develop this type of model. We have burnup ranging from about 25, 30 out to 63,000, with fluence ranges from about five to 12 ten to the 21. These oxide thicknesses range from rather low, on the order of ten to 15 microns, up to 110, 115, 120 type range with oxide spallation in some cases. Like testing temperatures range from room temperature all the way up to operating temperature type conditions. And then the strain range was all from very fast strain rates, on the order of five per second, all the way down to ten to the minus five per second. So quite a variety of strain rates.

Just to kind of point to a question or a comment that, Dana, you made earlier, in these oxide thickness ranges that I'm talking about here, these are generally the measured oxide on the sample that was tested in the mechanical property test. There are a variety of different tests that are done here. we have

CHAIRMAN POWERS: The question I'm going to ask you eventually, so you can think about it, you don't have to answer it right now --

MR. MONTGOMERY: Okay.

CHAIRMAN POWERS: -- is I see -- you know, I see in this topical report that you're going to develop critical strain energy density correlation as a function of the oxide thickness, and you're going to that with the Least Squares method, okay? And you're going to do that taking this oxide thickness or its ration to the clad thickness as a well-known parameter, yet the previous speaker said that there was substantial uncertainty in that oxide thickness, approaching 100 percent, as you got down to the lower thicknesses that you have here. Okay? And when you've got that situation where your independent variable is uncertain just as much as your dependent variable in your correlations, you can't use normal Least Squares fitting methods, you tend to overemphasize the slopes when you do that.

MR. MONTGOMERY: Okay. Thank you. I will think about that and try to answer it after lunch if we get that far.

Okay. Just to point out that generally the oxide thicknesses that I have reported in this table, and that we used in the next plot, were measured on the sample. Now, I did not get into the details of the error associated with the measurements themselves, but these are very local, as I was about to say. The ring tension specimens are generally a quarter of an inch in height. They're a ring and they're tested by pulling with some sort of dye device on the inside surface, maybe a double-D set pull. Axial tension tests are generally short four- to six-inch segments that are pulled axially. And then burst tests are generally six- to eight-inch specimens that are pressurized with either primarily oil but some of them are gas pressurization systems. Some have been included -- removed all the fuel, some of them have only removed part of the fuel. But you have a variety of different tests that we get the information from.

The next page gives us a flavor for a subset of this data. This is data all applicable to 300 degree C range. You see from 280 to 400 degrees C. What I've plotted here is the critical strain energy density which, in effect, is an integration of the stress/strain curve coming from the experiment, plotted as a function of the sample oxide thickness to cladding thickness ratio. We picked that particular parameter because in most of these samples the hydrogen concentration in itself is not measured. In some they are, but a good fraction of them they're not. And we know that really it's the hydrogen that's the variable that we want on the X-axis but since we don't have access to it, the oxide to thickness ratio was a parameter that, in effect, represents the hydrogen impact.

We have a variety of testing conditions. We've got axial tension test, ring tension tests, we have burst tests. We also have separated out the solid symbols are the data from samples that have spalling oxide layers on them. The samples themselves may not have come exactly from a spalled area or have exactly spalling on them, but they came from regions that had spallation. And that would be the solid symbols here. And you do see a separation between samples that were oxidized but without spalling and then those that are oxidized with spallation. So there is some separation of the data.

You see some scatter here on this plot, but a good part of that scatter is related to the test conditions. We're mixing different temperature ranges, we're mixing different testing conditions. We've tried to use biaxiality correction factor to bring together the burst data and the uniaxial type tests, so there has been some, it's been talked about in the topical, a correction factor that brings into the biaxiality effect between a burst and an axial test -- or a uniaxial test.

There is some scatter due to design effects. There's some bending effects that come into play in the ring specimens, for example, so there's some test artifacts that it will add some scatter to that.

Currently, I'm showing here a best fit of all the open symbols and non-spalled data and the a best fit of the spalled data. And you may wonder why we selected to use a best fit as opposed to some other lower bound or some other type of fit, and I'll talk about that in a minute about how we justified that by --

CHAIRMAN POWERS: See here's where the question comes up, is that you fit this with ordinary -- and yet your independent variable in the fitting process is just as uncertain as your dependent variable. And you should not do that. You should use something like a min-max sort of process, because otherwise you're going to overestimate slopes.

UNKNOWN: You eventually take a logarithm of this and do it with a linear by a Least Squares fitting.

CHAIRMAN POWERS: But you've got uncertainty in both variables.

MR. MONTGOMERY: I understand.

CHAIRMAN POWERS: And we can't use them in the ordinary linear Least Squares fitting.

MR. MONTGOMERY: Certainly, your point is well taken and we will go back and look at if we added error bars in the X direction on these, how big they would be with respect to what we did the fitting for. I'm not fully convinced yet that it's large enough to have a significant impact on the fitting process.

CHAIRMAN POWERS: Rosa told me that the oxide thickness measure in uncertainty are quite large, especially as you move toward thin oxides.

MR. MONTGOMERY: Thinner oxides. Now, a lot of these oxides were measured destructively, and what Rosa's referring to may be a non-disruptive poolside examination technique. There is a lot bigger variability in poolside examination techniques as opposed to destructive examinations. Here, primarily these were determined through destructive examinations, because the samples are defueled and tested in a hot cell and through metallography it's fairly straightforward to get the oxide thickness from the specimen, but not in all cases.

CHAIRMAN POWERS: I mean the problem is you can measure it at one location to three significant figures, but if in fact you have azimuthal and --

MR. MONTGOMERY: Azimuthal variations, yes.

CHAIRMAN POWERS: -- axial variations, that's what you really want.

MR. MONTGOMERY: Right.

CHAIRMAN POWERS: You want some volume with --

MR. MONTGOMERY: And that's what we -- I would go back -- taking your input, I would go back and look, what would be the variability for each sample? And we'd have 100 samples here and I'd go back and try to determine is that 50 plus or minus five or is that 50 plus or minus 25?

CHAIRMAN POWERS: Right.

MR. MONTGOMERY: That's what I would try to do.

MS. YANG: Robbie, I thought you had done analysis to show the uncertainty bar, how the effects the criteria.

MR. MONTGOMERY: Well, I'll --

MS. YANG: You can go into that later.

MR. MONTGOMERY: -- go into the uncertainty, but that's the next slide is that I've looked at different fitting approaches. Instead of doing a best fit, a lower bound fit to this database and then limiting the amount of data we used to look at just the burst data, so it fit just the burst data, some people would argue that's the most applicable to a PCMI stress state would be the burst data. So I've done that.

MS. YANG: Robbie, if I could just add one more thing, if you'd go back to your slide. I'd just say the uncertainty of ten microns that's at the poolside. If you ask the person using the eddy current technique, they probably would quote something like a couple micron that's the technique, but I think ten is a reasonable number. But for very think oxide, let's say the oxide is ten or 20 microns, the cladding ductility is so high it probably doesn't make much of a difference if you're talking about ten micron or 30 micron.

CHAIRMAN POWERS: It makes a huge difference when yo do Least Squares methods.

MS. YANG: Yes.

CHAIRMAN POWERS: Then you're waiting just as much on that end as you are on this end, and you shouldn't be doing it, it will flatten your curve. It's giving you a slope which may not exist.

MS. YANG: You are right about the fitting, but this curve is the data that we develop the CSED, but when we develop the criteria that we propose in the topical, we're taking an upper bound curve. So in that case, the uncertainty in the oxide thickness is not very important. I'm giving away a little bit of what Robbie is going to say, but I just want to point out the difference in the data when we develop the criteria, which we really take the upper bound of the corrosion thickness, so that in the case the uncertainty in the measurement of the oxides are not relevant. So we can come back to that when he presents the --

CHAIRMAN POWERS: I'll be stunned.

MR. MONTGOMERY: Okay. So I didn't put all the data on this but the blue line is the same as the previous slide where you saw the data scattered about. And in addressing the uncertainty question that we've -- and the data scatter question that has been raised before, we also looked at a number of other ways to look at the data, and that was with fitting just the burst data and ignoring the other data from ring and axial, and then also taking a lower bound of the ring and burst data and arguing that the axial data, since it's not in the direction of PCMI, we could not look at that. So I will come back to this with regard -- well, I think the next slides shows it. Okay.

Now, if we then go back and analyze each of the experiments from CABRI that we've done here, these are the UO2 tests, with -- we used FALCON, you could use SCANAIR, which is the French version of FALCON, or FRAPTRAN, and calculate what the maximum strain energy density is during the pulse event. And that's what I have plotted here is the strain energy, and you can think of it in strain or stress if you want but I'm using strain energy density here, for each of the experiments. So we've gone and analyzed the pulse, given the appropriate boundary conditions and burnup levels and oxide thickness, et cetera, et cetera, taken that into account and calculated for the actual experiment pulse what the SED would be for that cladding. And we've put those points on here, and that's what the symbols mean, as a function of the maximum oxide thickness divided by the cladding thickness ratio for that test specimen.

MEMBER FORD: Just for interest, where would Rep-1 be, just for interest?

MR. MONTGOMERY: In terms of oxide thickness ratio, it's right here, and in terms of the calculated SED at failure, it's about right here, just about a half, little less than a half. So it went way down here.

Now, if we now superimpose on these tests, and I should just point out that these two tests, RepNa-8 and RepNa-10, as Rosa talked about this morning, they did fail with a cladding crack.

MEMBER FORD: So just to follow up on that, I apologize for destroying your train of thought, based on that, Rep-1 is not crazily out of your model. Assuming that your red line is correct, and there's some assumptions in that, and given the variance you have on either side of that line, it's not out of line, especially if you put importance on any stress intensification, either because of that pit or because of the scratch. It's not so out of line.

MR. MONTGOMERY: Yes. it sits down in this range, and we would have to look and see what would be necessary in terms of stress intensifications or some other factors that would either move this line down or move it up if we were to do a local effects calculation.

MS. YANG: It's below the curve.

MR. MONTGOMERY: It's well below the curve. It's down in this range, approximately a half. Okay.

So I get the sense that at least some in the room are understanding what I'm trying to do here. So if we then take the previous curves, the CSED curves, and compare them, this is the best fit for the non-spalled material and this is the best fit for the spalled material. We see that for the failures, they reside above the spalled CSED so they would be predicted to fail by the analysis process. The non-spalled specimens, 2, 3, 4 and 5, all reside below the best fit. They survived without failure, and that's what this process would indicate.

Now, if we were to go to instead of the best fit, the best fit of the burst data, non-spalled again, we see that it would basically give almost the same answer as the blue line except that RepNa-2 would be predicted to fail. And then if we went to the lower bound of the data, we see that that curve would predict that RepNa-2 and 3 failed when in fact they did not. So you can see there's some justification -- the strongest justification for using a line more like this one is the fact that it does reproduce the experiment results.

And we've done this for the tests done in sodium, which is elevated temperature, 280 degrees C. And the process is similar when we -- I didn't show you the CSED data for that, but we've done it also for the room temperature tests. So with mechanical property data for temperatures less than 150 degrees C, we've derived a similar curve through another database, albeit not quite as large as the other one, and then analyzed some of the -- these are tests out of -- all these are from the Japanese program. The Japanese program is done in atmospheric condition in water, so you're starting at 25 degrees C. The SPERT-CDC test is the same way.

We see a similar correlation where the failures are near or above the line of the CSED, and those that did not fail are below the line. There are two that reside very near the line or on the line, which in post-test examinations they found part-wall cracks. So they were very near failure. They did not fail, but they were very near failure.

MEMBER FORD: And the physical argument is purely difference between those two cards is difference in temperature and therefore the ductility of the Zircaloy-4 with a given amount of hydride.

MR. MONTGOMERY: Yes.

MEMBER FORD: Hydriding being --

MR. MONTGOMERY: Yes, correct.

MEMBER FORD: -- with the oxide fitness.

MR. MONTGOMERY: Correct. So the primary difference between these two curves is the temperature effect on ductility. The hydrogen effect, which is influenced by temperature because of solubility considerations, drives the -- is the mechanism that drive the difference between those two lines.

So in the previous set of slides, I've established an analysis methodology that has been able to reliably reproduce the results of the experiments conducted on irradiated fuel material. And given this basis of understanding, now we understand the processes that go into cladding failure under power pulse condition. We can use that to now establish the licensing threshold for fuel rod failure. And so we've done that and that's in the topical report, and we did that to construct something that's consistent with the licensing approach. And what that means is we're going to derive a radial average fuel enthalpy at failure as a function of rod average burnup. There are other ways that it could be done, but this one is much more consistent with the approach where coming out of the 3-D neutronics calculation is generally a radial average fuel enthalpy, and so if we provide a threshold for which they can compare this coming out of the 3-D neutronics, that -- or the neutronics calculations, not necessarily 3-D, neutronics calculations, that now is a function of burnup. Before it was burnup-independent. So it's consistent with the methodologies that are established out there for licensing.

To address the uncertainties involved in the analysis methodology and the approach in general, we have elected to use a corrosion versus burnup correlation which has some conservatism built in. And that gives us a relationship between the cladding oxidation and the rod average burnup. And since we know the cladding ductility is a function of cladding oxidation, we can now have a ductility versus burnup relationship. And that's illustrated here.

So, in essence, what we've done to develop the fuel rod failure threshold is illustrated on this slide schematically. You've seen a bit about the CSED versus oxide thickness to clad wall thickness ratio. That's the data we have here. I'll show you in just a minute we have oxide thickness versus burnup data. We can combine these two together to give a ductility parameter CSED as a function of burnup now for different material conditions. I've illustrated here schematically for different alloys, potentially. And then given an analytical bases to calculate the fuel enthalpy and the cladding response, we can then determine what fuel enthalpy level is needed to reach this CSED as a function of burnup. And that then derives the threshold that you saw a few minutes ago that Rosa presented.

CHAIRMAN POWERS: Let me come back to the plots that you were doing beforehand. I just glanced through your topical report and I did not find a tabulation of the data you used to prepare those plots of strained energy density versus the ratio. Would it be possible to get those tabulations?

MR. MONTGOMERY: We're working on putting that together.

CHAIRMAN POWERS: I'd appreciate getting a copy of that.

MEMBER FORD: Actually, I've done the same -- I'm trying to follow your argument because you're going back. On this plot here where you plot strain energy density versus oxide, in order to get to that plot and to put on the data points that you have for Rep numbers, you also need the relationships between burnup and enthalpy and strain energy density. Those are all separate algorithms you need to get to how you place those --

MR. MONTGOMERY: Yes. Correct.

MEMBER FORD: -- points on that plot. You haven't shown those, have you?

MR. MONTGOMERY: No, I did not go into details of that.

MEMBER FORD: Okay.

MR. MONTGOMERY: But I'll briefly explain it. We take a fuel transient behavior code, FALCON is the one we use, and we analyzed each one of these experiments, providing as input the power pulse shape, the burnup conditions, so we have to do a steady state analysis up to each burnup. The burnup ranged here from 30,000 to 65,000 depending on which experiment we're looking at here. So we defined the initial conditions of each experiment which brings in the burnup from the post-test examinations, the pre-test examinations as well. All that is brought into initialize the transient analysis. The transient analysis with FALCON is done, and that value of SED that's plotted there comes from that analysis.

MEMBER FORD: But each of those calculations there's got to be a certain amount of uncertainty, uncertainty in terms of the validation of the various codes against data. And is it possible that the reasonable correlation you have there between the data and the theory, or the computation, is luck? Is that all being too cruel?

MR. MONTGOMERY: I would like to not say that it was luck. I haven't gotten into details of the code of the validation base of the code and the numerical bases of the program. The approach that we're using here has been replicated by others. The French, using SCANAIR, have done something similar and the results are very consistent. I'm not showing those, but I can get you that information.

MEMBER FORD: Okay.

MR. MONTGOMERY: So I don't believe there's a large element of luck in here. There may be a small element of luck in here, but I don't believe there's a large element of luck.

MS. YANG: If I can add, I think Robbie there published a paper that shows the comparison between what the code predicted in terms of the deformation, in terms of measured deformation and predicted deformation, and I think that answers your question.

MEMBER FORD: So there is experimental validation for those --

MS. YANG: Yes.

MEMBER FORD: -- algorithms that go into --

MS. YANG: Yes.

MEMBER FORD: -- it and make it that way.

MS. YANG: Yes.

MR. MONTGOMERY: Primarily for the rods that did not fail they have measured post-test examinations for things like cladding strain deformation, radial strain and hoop strain and axial. So they have those types of data that I have not shown which we have --

MS. YANG: Have been published.

MR. MONTGOMERY: Have been published and the code comparisons to it are reasonably well.

MEMBER FORD: I'm sorry, also I'm just flipping through your charts. You're going to go into how you're going to use this --

MR. MONTGOMERY: Yes.

MEMBER FORD: -- from this point on. Would you mind going back two more plots to the one that you have the "night sky." The reason I call it "night sky" from the cracking world we have a lot of "night sky" plots look like this. The presumption here is that there is a unique relationship between crack strain energy -- or critical strain energy density and oxide cladding thickness and that there's just one relationship, that's that line. But in fact there's got to be more than just a single parameter relationship.

MR. MONTGOMERY: Well, we know the temperature for sure.

MEMBER FORD: The temperature and the

strain rate. Even though you say strain rate is not a big thing, it will be. Physically, it must be an input to the model.

MR. MONTGOMERY: In looking at this data under a variety of strain rates, we didn't find a strong strain rate dependency. Now, we have included in this a strain rate dependency, so there is a -- the biaxiality factor that we used to relate the axial and ring tension has a strain rate effect. So we have that. There is some inherent strain rate built in.

MEMBER FORD: I guess the reason I'm bringing it up is we see a lot of plots like this out in literature and the correlation factors must be very low on that blue line. And yet it's the basis for all of your subsequent analysis and the use of that analysis, and it just makes me feel uncomfortable that we have no way of knowing how to normalize or collapse that to correct, if you like, those data points even though there are experimental errors on each data point, how you correct those data points to move it down towards that blue line if that blue line is correct.

MR. MONTGOMERY: Well, the only thing that we have done, as I said, we have gone through and looked at this various looking at the data to try to bound it, to try understand the uncertainty and impact of uncertainty. So we've looked at this. We see in this slide where that -- how that uncertainty could influence at least the validation process.

MEMBER FORD: Okay.

MR. MONTGOMERY: And then, as I'll go into later on, in the application, we've also looked at this uncertainty variation on the result of the application and we come up with a threshold and how big of an impact this variability would be on the threshold that's derived in application of the methodology. So we recognize that there is clearly scatter inherent in that data that adds some uncertainty into the process that we're implementing. And we tried to address it through this evaluation. And I'll talk at the end and show that at low burnup where the oxide thickness is lowest and you see the biggest impact, the effect is there but it's not that large. It can be on the order of ten calories per gram or so, but here in the area where these all tend to converge because the data is getting tighter together the impact is much smaller.

MEMBER FORD: Okay.

MR. MONTGOMERY: Okay. Let's see, where was I now? We're talking about how we use this methodology, combined with the data, to come up with the threshold value. Let's see, so a part of this process is the requirement of an oxide thickness versus burnup relationship. So we've collected several thousand poolside examination measurements on oxide thickness and looked at the data and there's clearly a trend in the data that as the burnup increases the oxide is increased. Now, there's a lot built into that, there's duty effects, the temperature of the plant effects, many things other than burnup, but we've boiled it down to burnup for this application.

And in looking at the scatter and the variability in the oxide thickness versus burnup, we elected to take a very conservative approach and just take a trending line that mirrors, to some degree, the relationship of burnup versus -- oxide versus burnup so that we can bound some of these higher points and then prescribe a limit of 150 microns to preclude the possibility of oxide spallation. We know that above 100 microns the propensity for oxide spallation tends to increase because of the internal stress effects and other effects that influence the spallation process.

So in our application of the methodology, we're applying this very conservative oxide thickness versus burnup curve. It's anticipated strongly with advanced alloy materials for the cladding, as I said, designed to go to high burnup that you'll fall well below that curve. So you'll be in this -- well below the curve and the envelope of operation down in here.

So here's the bottom line. I'm sure you're going to have lots of questions of how I got there. But, essentially, the result of all this process is a radial average peak enthalpy that is essentially 170 calories per gram out to a burnup level and then becomes a function of burnup after that. So from about 36,000 on it's now a function of burnup. Below, it's burnup-independent. The 170 calorie per gram limit comes from the DNB failure process. Experimental data from tests show that below 170 calories per gram the cladding temperatures do not exceed that necessary to induce high temperature failure processes. So the failure would only occur above this line and appears where you get to the very high temperatures needed to fail the cladding.

PCMI, because of changes in the ductility function that we've used, combined with the gap closure effects, begins dominant after 36,000 and then begins to saturate out as you reach the 100 micron level.

MEMBER ROSEN: This is excellent, because what this is, as a utility guy, I can run to 100 gigawatt days per metric ton because it saturates out.

CHAIRMAN POWERS: No. It seems to me that there's some flaw here that he comes up and he says, all right, at 40 gigawatt days per ton I don't want the material to spall and I know that oxides do get spalling, so I'm going to cap my correlation. Then he calculates this curve. His curve should come up to 40 gigawatt days per ton and then stop. He should say you have to stop at 40 gigawatt days because there's the potential of spalling and you switch to a different curve then.

MR. MONTGOMERY: We're saying that the oxide is below this level, and we are going to draw at envelope at which you're below. We're not saying that because --

CHAIRMAN POWERS: Starting at 40 gigawatt days, that philosophy disappeared.

MR. MONTGOMERY: That becomes the envelope. As long as you're below 100 microns --

CHAIRMAN POWERS: You now switch to a different criterion. As soon as you cross 40 gigawatt days per ton, you're saying, "Oh, yes, but in addition to this, you have to stay below 100 microns."